EORIES
IN ASTRONOMY

W. STIRLING

NEW THEORIES IN ASTRONOMY

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BY

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TO THE READER.

MR. WILLIAM STIRLING, Civil Engineer, who devoted the last years of his life to writing this work, was born in Kilmarnock, Scotland, his father being the Rev. Robert Stirling, D.D., of that city, and his brothers, the late Mr. Patrick Stirling and Mr. James Stirling, the well known engineers and designers of Locomotive Engines for the Great Northern and South Eastern Railways respectively.

After completing his studies in Scotland he settled in South America, and was engaged as manager and constructing engineer in important railway enterprises on the west coast, besides other concerns both in Peru and Chile; his last work being the designing and construction of the railway from the port of Tocopilla on the Pacific Ocean to the Nitrate Fields of Toco in the interior, the property of the Anglo-Chilian and Nitrate Railway Company.

He died in Lima, Peru, on the 7th October, 1900, much esteemed and respected, leaving the MS. of the present work behind him, which is now published as a tribute to his memory, and wish to put before those who are interested in the Science of Astronomy his theories to which he devoted so much thought.

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NEW
THEORIES IN ASTRONOMY.

INTRODUCTION.

THAT a little knowledge is a dangerous thing to the possessor, has been pointed out often enough, probably with the idea of keeping him quiet, but it is very certain that the warning has not always had the desired effect ; and in some respects it is perhaps much better that it has not, for it is sometimes the case that a little knowledge exhibited on an inappropriate occasion, or even wrongly applied, throws light upon some subject that was previously not very well understood. It sometimes happens that unconscious error leads to the discovery of what is right. The fact is, all knowledge is at first little, so that if the first possessor of it is kept quiet there is little chance of its ever increasing. On the other hand, much knowledge seems to be quite as ready to become dangerous on occasion, for it has sometimes led its possessor to fall into errors that can be easily pointed out, even by the possessor of little, if it is combined with ordinary intelligence. The possessor of much knowledge is apt to forget, in his keen desire to acquire more, that he has not examined with sufficient care all the steps by which he has attained to what he has got, and that by placing reliance on one false step he has erected for himself a structure that cannot stand ; or, what is worse perhaps, has prevented those who have followed him in implicit dependence on his attainments and fame from finding out the truth. If, then, both of these classes are liable to fall into error, there appears to be no good reason why one belonging to the first mentioned of them should absolutely refrain from making his ideas known, especially as he may thus induce someone of the second to re-examine the foundations on which he has built up his knowledge.

These reflections are in greater or lesser degree applicable to all knowledge and science of all kinds, even theological, in all their individual branches, and can be very easily shown to be both reasonable and true. And it may be added, or rather it is necessary to add, that every one of all the branches of all of them has a very manifest tendency towards despotism ; to impose its sway and way of thinking upon the whole world.

At various intervals during the present century speculation has been indulged in, and more or less lively discussion has taken place about the great benefit it would confer on universal humanity, were all the weights and measures of the whole earth arranged on the same standard. The universal standard proposed has been, of course, the metrical system, which had been elaborated by French *savants* who most probably thought they had arrived at such a state of knowledge that they were able to establish the foundations of all science of all kinds and for all time, upon the most sure and most durable principles. These periods of metrical fever, so to speak, seem to come on without any apparent immediately exciting cause, and some people succumb to the disease, others do not, just the same as in the cases of cholera, influenza, plague, etc. Whether some species of inoculation for it may be discovered, or whether it will be found that an unlimited attack is really perfect health, will most probably be found out in the course of time, although it may be some centuries hence. What is of interest to understand at the present time is, what are the benefits to be derived from the proposed universal standard of weights and measures, and how they are to be attained.

The principal and most imposing reason for its adoption is that it would be of immense service to scientific men all over the world, who would thus be able to understand the discourses, writings, discoveries, etc. of each other without the necessity of having to enter into calculations of any kind in order to be able to comprehend the arithmetical part of what they have listened to or read. Another argument brought forward in favour is, that it would greatly facilitate commercial transactions with foreign countries; and it has been lately advanced that great loss is suffered by one country selling its goods, manufactured according to its own measures, in countries where the metrical system has been adopted. Yet another advantage held out is the convenience it would be to travellers in money matters; but as this argument cannot be admitted without taking into consideration the necessity for one universal language all over the world, it has practically no place in any discussion on the subject, until the evil caused by the building of the Tower of Babel has been remedied.

Not long after one of the periodical attacks of metric fever we came upon an essay written by J. J. Jeans on "England's Supremacy," and published in New York by Harper and Brothers, in 1886, in which we found the following:—

Numerical relation of occupations in England and Wales in 1881:

Professional	2·5 per cent.	Commercial	3·7 per cent.
Domestic	7·0 „	Industrial	24·5 „
Agricultural	5·3 „	In all	43·0 „

This statement shows that 43 per cent. of the whole population are occupied in some business or work of some kind, and leads us reasonably to suppose that the remaining 57 per cent. consist of women, children, and people who—to put it short—are non-producers; the whole of whom can hardly be considered as much interested in the making of any alterations in the weights and measures of their country, rather the contrary, for they cannot expect to be much benefited by any change.

The professional class naturally comprehends Theology, Law, Medicine, and Science generally, so that the 2·5 per cent. ascribed to it would be seriously reduced, if the advantage derived from the desired change were reckoned by the number really benefited by it. A similar reduction would have to be made on the 3·7 per cent. stated to be occupied in Commerce, as it is not to be supposed that the whole of the number are engaged in foreign trade. Thus the number of people in these two classes who might really reap some advantage from the change, may be reduced by at least one half; and if we consider that one person in ten of those occupied in the Agricultural and Industrial classes is a scientist—we may pardon the Domestic class—a very liberal allowance indeed, we arrive at the conclusion that 6 per cent. of the whole population might find, some more, some less, interest in the introduction into our country of the French metric system.

The above statement refers only to England and Wales, but if Scotland and Ireland are added to them, the 6 per cent. proportion could not be very greatly altered: perhaps it would be less favourable to the change. Thus 94 per cent., or something like 37 millions, of the whole population of the United Kingdom would be called upon to change their whole system of weights and measures, in order that 6 per cent., or somewhere between 2 and 2½ millions, should find some little alleviation in a part of their labours; and surely 2 to 2½ millions of scientists and merchants engaged in foreign trade is a very liberal allowance for the population of our country. If this does not show a tendency towards despotism, it would be hard to tell what it does show.

Of course, it would not be fair to assume that the whole of the 6 per cent. would desire to see the proposed change carried into effect. In all likelihood, a very considerable portion of the number would be disposed to count the cost of erecting such a structure before actually laying its foundations, and would refrain from beginning the work on considering by what means it was to be brought to a conclusion; even without going so far as to find out that 94 per cent. of it at least would have to be done by forced labour. They might even go the length of speculating on how long it would take to coerce the 94 per cent. into furnishing the forced labour, and on the hopelessness of the task. On the other hand, they might think it more natural to lay hold of the alternative of adopting a special system of weights and measures for the use of Science and Foreign Commerce alone, and leave the 94 per cent. to follow their own national and natural customs, which they would be very likely to do whatever might be determined, if we may judge by the progress made in France a century after the system was thought to be established. Very little opposition could be made to such a course, and if the best possible system were not adopted, the scientists would be the only parties put to inconvenience. They could improve and reform it, should they find it not to be perfect, without the necessity of coercing the 94 per cent. into furnishing another contingent of forced labour. But little is to be gained by saying any more about it. Should the metrical system be adopted some day by Act of Parliament, Science will have obtained what it has so long coveted, will be quite satisfied, and will trouble itself very little about how it affects the rest of the population. It will perhaps never even think of how India will be brought to buy and sell through the medium of the French Metrical System.

And now we have only one step to take on this subject. We may say that the project of establishing one standard of weights and measures for the whole world has a most unpleasant resemblance to the object proposed by the builders of the Tower of Babel; the only thing that can be said in its favour being that it points towards an endeavour to do away with the bad results produced by that enterprise and to bring matters back to the state the world was in before the foundations of that celebrated edifice were laid.

The foregoing is only one instance of the many that could be cited where science has schemed projects for universal progress without due thought, and has come to the conclusion that they could be easily carried out. There are as many examples of this jumping at conclusions as would fill many books, which of course it is not our purpose to do; but there is one that it is necessary to have brought forward for examination, because of its having, through a most incomprehensible want of thought, a tendency to establish Natural Religion on the very bases upon which the Christian Religion is established.

The one referred to is that by which some of the most eminent scientists of the present century, following up what was done in former times, have been able by deep study and experiment, unfortunately coupled with unaccountable blindness or preconceived erroneous ideas, to formulate processes by which the whole universe may have elaborated itself from protyle and protoplasm, or some such substances which, without any foundation to build upon, they suppose to have existed from all eternity. This advance in science has been called the Theory of Evolution, and has been very generally considered to be new, or of comparatively very recent conception; but it is only a piece of the evidence of a very general propensity in those who come to acquire a little more knowledge, to flatter themselves that they have power to seize hold of the Unknown.

The theory may be *new*, but evolution most assuredly is not, as any one may convince himself who will take the trouble to read the first chapter of the Book of Genesis *and to think.* There he will find it stated that the earth and all things in it and on it were created and made in six days, or periods of time, showing him distinctly, if he does not shut his eyes wilfully, that two operations were employed in the process, one of creation and the other of making, which last can mean nothing but *evolution.* It does not matter a straw whether the latter operation was carried on personally by the Creator and Maker, or under the power of laws ordained by Him for the purpose; it was evolution all the same, and just the kind of evolution the scientists above alluded to would have us believe to be new, not far from 3500 years after the account of the creation and making of the world was written by Moses.

It will do no harm to take special notice of the work that was done in each of the six periods, as it will help to fix attention on the subject during examination and judgment; and may even tend to open the eyes of any one who had made up his mind to keep them shut.

In the first period the heavens and the earth were created, but the earth was without form and void, *inanis et vacuus*, according to *The Vulgate*—(does that mean empty and hollow?)—and darkness was upon the face of the deep; but light was *let* shine upon the earth to alternate with darkness, and between the two to establish day and night. It is therefore evident that after the earth was created it had to be reduced to something like its present form, a globe of some kind, and to rotate on an axis, otherwise there could have been no alternations of light and darkness, of day and night. Where did the light come from? Some people seem to think that Moses should have included a treatise on the creation and evolution of the universe, in his account of the work done in the first period of creation. For all that can be truly said to the contrary, he seems to have been quite as able to do so as any scientist of the present day; but it is evident he thought it best to limit himself to writing only of the earth, as being of most interest to its inhabitants, and enough for them as a first lesson. The literature of science, however, of the present day, will tell them that long ages after the earth was *evolved* into a globe, it must have been in a molten, liquid state, surrounded by an atmosphere of vapours of some of the chemical elements so dense that no light from without could shine through it, and could only be penetrated by light after the cooling of the earth had dispelled a sufficient portion of that dense atmosphere. With this explanation, which they had at hand for the looking for, they might have been so far satisfied, and have left Moses to tell his story in his own way.

In passing, it may not be out of place to say that, after the cooling of the earth had proceeded so far that the vapours of matter had been condensed and precipitated on its surface, all boiling of water whether in the seas or on its surface must soon have ceased, so that no inconceivably enormous volumes of steam could be thrown upwards to maintain an atmosphere impenetrable to light; and that when dense volumes of steam ceased to be thrown up, the condensation of what was already in the atmosphere would be so rapid, and its density so soon reduced sufficiently to admit of the passage of light through it, that one can almost fancy himself present on the occasion and appreciate the sublimity of the language. "And God said, Let there be light, and there was light"; more especially if he had ever stood by the side of the cylinder of a large steam engine, and understood what he heard when the steam rushed from it into the condenser, and noted how instantaneous it seemed to be. Any one who has watched a pot of water boiling on the fire and emitting clouds of steam, will have noticed how immediately the boiling ceased whenever the pot was removed from the fire ; but he will also have noticed that the water still continued to emit a considerable quantity of vapour, and will be able to understand how it was that the cloudy atmosphere of the earth, at the time we are dealing with, could allow light to pass through it but still keep the source of light from being visible. He experiences daily how thin a cloud will hide the sun from his sight. But there is more to be said about this when the time comes for taking note of the actual appearance on the scene of the sun, moon, and stars.

To obtain some rude idea of the time to be disposed of for evolution during the first period, let it be supposed that the whole of the time consumed in the creation and development of the earth was 300 million years, as demanded by some geologists, the first period of the six would naturally be somewhere about 50 millions of years, a period which would allow, probably, very liberal time for evolution, but could never have been consumed in creation, seeing that creation has always been looked upon as an almost instantaneous act. And if anyone is still capable of exacting that the period was a day of twenty-four hours, he has to acknowledge that at least twenty-three of them were dedicated to the work of evolution.

The second period was evidently one solely of evolution, as all that was done during it was confined to *making* the firmament which divides the waters from the waters ; an operation which could never be confounded with creation, being probably brought about solely by the cooling of the earth, which was the only means by which a separation between the waters covering the earth, and those held in suspension above it by the atmosphere, could be brought about, and must have been purely the work of evolution.

The third period was begun by collecting the waters under the firmament into one place and letting the dry land appear ; which, it may be well to note, gives it to be understood that the surface of the solid part of the earth had come to be uneven either by the elevation or depression, perhaps both, of some parts of it, and next the earth was *let* bring forth grass and trees, and in general vegetation of all kinds. These cannot be considered otherwise than as operations of evolution: there was no creation going on beyond what may have been necessary to help evolution, and of that not a word is said. Here it is well to notice that until the waters were gathered together into one place and the dry land appeared there could be no alluvial deposits made in the sea, and that till well on into this third period, that is well on for 150 million years from the beginning, there could be no geological strata deposited in it containing vegetable matter, for the very good reason that although rains and rivers may have swept earthy matter into the sea, the rivers could not carry along in their flow any vegetable matter until it had time to grow.

Should evolutionists think they have discovered something new in spontaneous generation, we refer them to the 11th verse of the chapter, where they will see—"And God said, Let the earth bring forth grass, the herb yielding seed, and the fruit-tree yielding fruit after his kind, whose seed is in itself, upon the earth." The conclusion of this passage asserts plainly that the seed was already in the earth, somehow or other, ready to germinate and sprout when the necessary accompanying conditions were prepared. The words are very few, and they can have no other meaning.

In the first period "God made two great lights: the greater light to rule the day and the lesser light to rule the night; he made the stars also." This passage has been "a stumbling block and rock of offence" to some people possessed of much knowledge and to some possessed of little; the one party professing to disbelieve all because the sun was *made* four days after there was light, and the other party, supposing that there might have been light proceeding from some other source during the first four days. Both parties seem to have forgotten that the earth was created without form and void, and that being so the same would naturally be the case with the sun and the moon; all of them had to be made into form after their creation. By what means? By evolution, of course, or whatever else anyone chooses to call it; that will make no difference.

As far as it can penetrate into the mysteries of creation, Physical Astronomy has endeavoured to show how the solar system may have been formed out of a mass of nebulous matter. Furthermore, as has already been adduced in evidence, that at one time the earth must have been a molten, liquid globe surrounded by vapours of metals, metalloids, gases, and finally by water; and even goes the length of supposing that the planets were evolved to something approaching their present state, long before the sun attained its present form. Following up this hypothesis, it is more than probable that the sun had not attained that form when this fourth period began, and, although capable of emitting light early in the first period, still required a vast amount of evolution to reduce it to the brilliant globe now seen in the heavens. Everybody knows that plants grow without sunshine, and it is generally believed that the primary forests of the earth grew most rapidly in a moist, stifling atmosphere, which neither admitted of animal life, nor could be penetrated by sunshine. Thus Physical Astronomy cannot say that the sun could not have been made into its present state until near the end of this fourth period. It *may* have been as bright as it is now, though very probably not, as we shall see in due time; but it could not *shine* upon the earth, neither could the earth, nor anything thereon, see it. It is not necessary to say anything about the moon, as it only reflects sunlight, and the reflection could not reach the earth if the light could not.

In the fifth period the waters were *let* "bring forth the moving creature that hath life, and fowl that may fly above the earth in the open firmament of heaven." Here again spontaneous generation may have been provided for beforehand, the same as in the case of vegetation. Also it is said "God created great whales," and it is to be observed that this is only the second time that creation has been mentioned in the book, and would seem to teach that *making*, or evolution, was the most active agent at work in the construction of the earth—and, we may add, of the universe.

The sixth period was one almost exclusively of evolution, unless it should be considered that spontaneous generation is a different, and newly discovered process. In it God *made* the beast of the earth, cattle, and everything that creepeth upon the earth, after his kind. Last of all: "God said, let us make man in our image, after our likeness." Thus it appears that the only work of creation done in this period was that of creating man, and even that *after* some length of time and work had been expended in *making* or *evolution*, which may have extended over a very considerable portion of the fifty millions of years corresponding to it.

We have supposed the work of creation to have extended over three hundred million years to satisfy some geologists, but our arguments would not be affected in any way by the time being reduced to the limit given by Lord Kelvin to the heat-giving power of the sun in the past, which he has made out to be between fifteen and twenty million years. That would only limit our periods of evolution to two and a half or three million years each ; each of them quite long enough to be totally inconsistent with our ideas of creation, which conceive of this as an instantaneous act. But although Lord Kelvin has in rather strong terms placed this limit, he at the same time says that it could by no means exceed four hundred million years, which is one-third more than we have calculated upon. Neither can our arguments be affected in any serious way by our dividing the periods into fifty million years each ; these may have varied much in length, but whatever was taken from one would have to be added to the others.

Furthermore, we may be allowed to say that fifteen to twenty millions of years of the sun's heat at the rate it is now being expended, can be no reliable measure of the time required for the operations of geology, for the reason that its heat must have been emitted in proportion to the quantity it possessed at any time. When it was created without form and void as no doubt it was, the same as the earth, it would have no heat to emit, but that does not mean that it possessed no heat until it was formed into the brilliant globe that we cannot now bear to turn our eyes upon. Even when it became hot enough to show light sufficient to penetrate the "darkness that was upon the face of the deep," it may still have been an almost shapeless mass, and have continued more or less so until it was formed into the body of the fourth period, which may even then have been very different from what it is now. Thus geology would have not far from one hundred and fifty million years in which a very small fractional part of the sun's emission of heat would suffice for its operations. But we shall have more to say on this subject when the time comes.

It being, therefore, a matter beyond all question—to people possessed of the faculty of thinking, and of candour to confess that they cannot help seeing what has been set plainly before their sight and understanding—that the opening chapter of the book of Genesis plainly teaches that making—evolution—had a very large and active part to perform in the creation of the universe and—much more within our grasp—of the earth; we can come to the conclusion that the theory of evolution, instead of being new and wonderful, comes to be almost infinitely older than the everlasting hills, without losing any of its power of inspiring inexpressible wonder.

Looking back over the examination into the first chapter of the book of Genesis we have just concluded, we cannot conceive how it could ever have entered into the thoughts of man, that the state of vegetable and animal life on the earth, at the present day, must have been brought about by continual and unceasing acts of creation, when creation has been mentioned only on three occasions during the whole process described in the chapter we have analysed, that is, 3 out of 31 verses; and while the other processes which we have brought forward—making and spontaneous generation—have never been alluded to, perhaps not even thought of.

We have no desire, neither are we qualified, to follow up this subject any further, but we have still one or two things to bring into remembrance.

One of the most illustrious of the founders of the Theory of Evolution has based his dissertations on the Descent of Man, on the Variation of Animals and Plants under Domestication, and on their *wonderful plasticity under the care of man.*

Here there is an explicit acknowledgment of the necessity for the direction of an intelligent guiding power to produce such variations; these never having any useful or progressive results except under such care. If, then, there is a necessity of such directing and guiding power in the case of variations of such inferior importance, the superintendence of some similar power must have assuredly been much more necessary for the creation and evolution of matter, of life, and of man himself. This is what, one would think, common sense and reason would point, and what the Theory of Evolution seems to think—evidently without studying the subject far enough; but all that it has been able to do has been to substitute Nature for the Creator to whom Moses has ascribed not only *Creation* but the *Making—Evolution*—of the universe.

This naturally leads us to speculate on what Evolutionists consider Nature to be, and as none of them—nor anyone else—as far as we know, has ever thought it necessary to define Nature, we have to endeavour to draw from their writings what, in some measure and some way, they would like us to believe it to be. We find, then, that the base of their operations seems to be Natural Selection, which can hardly be interpreted in any other way than by calling it the Selection of Nature. Thus, then, they apparently want us to look upon Nature as the *First Cause*. But, if Nature can select, it must be a being, an entity, a something, that can distinguish one particle of matter from another, and be able to choose such pieces of it, be they protyle or protoplasm, and to make them unite, so as to form some special body, organic or inorganic. It is plain, also, that Selection can only be performed by such a being, or something, such as just so far described, that can distinguish, choose, and arrange the particles of matter destined to form the very smallest body or the universe. Thus we see that in whatever way the basis of the Theory of Evolution is looked upon—*even for its own evolution*—there is required a being of some kind that has knowledge and power to evolve or make all things that are "in heaven above, or in the earth beneath, or in the waters under the earth." So we see that, if the theory of evolution dethrones the Creator and Evolver of the first chapter of Genesis, it has to enthrone another god which it calls Nature ; and has to get rid of that god, and any number of others, before it can be what it pretends to be.

We are all very voluble in talking of Nature, and enthusiastic in admiring its beauties, wonders, and wisdom, but it seldom occurs to us that we are really doing so without thinking of whence come the beauty, wonders, and wisdom. We must, therefore, not be too hard on evolutionists, as they have only done what we all do every day of our lives ; but if the theory of evolution is to be looked upon as a branch of science, we would recommend its students to open their eyes and think of it as a process which has been in existence from the beginning of things at least, and not as one of their invention or discovery. They may be able some day, through more accurate study and more convincing argumentation than they generally use, to lay claim to having discovered, as far as it is possible for man to do, the *modus operandi* of evolution, but that is all, and we would also warn some of them to think that, when we see them in their highest flights of science, genius, and self-sufficiency, we can

"Conceive the bard the hero of the story."

We have read a good deal of what has been called the War of Science, without having been able to see that there ever was any cause for such a war, with the exception of ignorance.

If Theology had been able, or rather had taken the trouble, to study thoroughly the first chapter of Genesis, and thus to comprehend that, if the earth was created without form and void, a great deal of work had to be done, after creation, in forming it into its present condition, there was no call upon it to find fault with Copernicus or persecute Galileus, because they said the earth revolved round the sun ; more especially as they do not appear to have ever said anything against religion or revelation. Neither was there any necessity for opposing the so-called new science of evolution, because it

(Theology) ought to have seen that the work expended in reducing the earth into form could hardly be conceived of otherwise than as a process of evolution; and would thus have been in a position to tell the authors of the *new* science that they had only discovered what had existed before the beginning of time.

On the other hand, there was no occasion for Science to take up the war. If it, in its turn, had taken the trouble to study and understand the first chapter of Genesis, it could have shown Theology that *it* did not comprehend, and could not give a true account of what religion and revelation are; whereas it (Science) seems to have had a strong tendency to demonstrate that religion and revelation are altogether false, and that the great work it has to perform is to dethrone Theology, and set itself up it in its stead.

It is not worth while even to think of who or which was the aggressor, seeing that the war originated from ignorance caused by want of thought and study on both sides. All that has to be said on the subject reduces itself to the fact that both Religion and Science have been coming, and are at present going, through the process of evolution. Can anyone say that Science has been truly scientific, without ever incurring in error, from the beginning of history up to the present day? Will any one venture to maintain that there has been no evolution, no progress, no softening of the spirit of Religion, from the institution of Christianity up to the end of the nineteenth century? If such there be, let the one look back to the time of Aristotle, and the other to the establishment of the Church under Constantine.

There has been for long an opinion, which goes on increasing in strength, that Science will ultimately reform Theology and put Religion in its right place; but if such is to be the case, Science has to begin by reforming itself and putting an end to error it has been, in many cases, teaching for generations; and by ceasing to formulate new theories, or bases of progress, which can be in many cases exploded by suppressing some of the error just alluded to. Little advance is made in science by forming hypotheses and theories, however brilliant they may appear, unless they are carefully studied and thought out to the very uttermost; because, if published abroad on the authority of some celebrated or even well-known name, they have a tendency to stop further investigation, and prevent students from exercising their own judgment and perhaps discovering what they might possibly find out were they to study them to the very end for their own satisfaction. This is in some measure the case even with respect to the solar system. We believe it can be shown that a more complete knowledge and comprehension of it, and even of the universe, has been kept back by the unquestioning acceptation by successive astronomers of the ideas and conceptions of their predecessors.

We have to acknowledge, at the same time, that Astronomy could not start into perfection at once, any more than any other science, and it is not to be wondered at that in times past ideas relating to it should have been formed without being properly thought out; even ideas that could not be properly thought out to the end for want of the requisite knowledge. But it is much to be regretted that such ideas should continue to be published at the present day as trustworthy instruction for readers who may look upon it as strictly correct. Among those who read text-books even on Astronomy, there must be a very considerable number who are rather surprised when they see statements made which do not agree with what they were taught at school, or with what they have practised in other sciences in their own professions or trades. It may be said that any person of ordinary intelligence will easily be able to correct such errors, but the evil does not stop here. If he can really correct them he will most probably find as well, that his instructors have been led into more serious errors, perhaps in more important matters, founded on the ideas which they had not fully studied out before giving them a place in their books. He may also find sometimes, in his reading, such ideas brought forward to substantiate some theory, just as far as they are required and then dropped, while a step or two further forward in the examination of these same ideas, would have exploded the theory altogether;

because, although founded to a certain extent on one law of nature, they are in contradiction with what is laid down in some other law.

The above will be looked upon as an unwarrantably bold assertion ; but a careful study of, or attention to, what is taught in the most advanced works on the solar system, even in science generally, will show it to be perfectly true. It is not only true, but the consequences of its being true have been much more serious than will be readily believed. In our own endeavours to understand what we had been reading, we have seen that some of the notions presented to us were only half formed, and that they have led to theories being founded which could never have been entertained at all had they been thoroughly studied out. More than that, they have prevented the truth from being arrived at in the fundamental conceptions of the construction of the earth, and, as a natural consequence, of the whole solar system, perhaps even of the whole universe.

There are probably many, even a great many, people who have arrived at the same conclusions as we have, but as far as it has been in our power to search into the matter, we have met with no attempt from any quarter to put an end to this defect in the literature of science ; perhaps because the work has the appearance of being too great to be readily undertaken, and also because it may be thought that there is little to be gained by it—as all is sure to be set right through time. But, as we believe that it will be beneficial immediately, in the case of the earth and solar system at least, we shall first attempt to show what are some of the defects alluded to, and then what knowledge may be acquired through their removal.

CHAPTER I.

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BEFORE astronomers could begin to determine the relative distances from each other, and the relative dimensions and masses of the various members of the solar system, they had to establish scales of measurements appropriate to their undertaking. This entailed upon them, of course, the necessity of determining the form, the different circumferences and diameters, and the weight of the whole earth, as any other scales derived from the only available source, the earth, would have been too small to give even an approximate value of the measures and masses to be sought for.

History tells us that at least one attempt had been made, over two thousand years ago, to find the circumference and necessarily the diameter of the earth, but it says nothing of any to ascertain its weight. There may have been many to determine both diameter and mass, but we know nothing of them; and when we think seriously about this, we cannot help feeling somewhat surprised that no attempt had been made to find out the density and mass till more than a century after Sir Isaac Newton's discovery of the law of Attraction, or Gravitation, as it is more usually called. But perhaps this is an idea that could only occur to one who has been *spoilt* by witnessing, in great measure, the immense strides in advance that have been made during the nineteenth century in science of all kinds, and does not duly take into account the immense labour, and the incessant meeting with almost insurmountable difficulties, that astronomers have had to encounter and overcome between the birth of modern astronomy and the end of the eighteenth century. Indeed, the difficulties can hardly be looked upon as altogether overcome even yet, as efforts are still being made to find out the exact distance of the sun, and it is not impossible that some small difference may be found, plus or minus, in the density at present adopted for the earth of 5·66 times the weight of water.

The geometer who, more than two thousand years ago, set himself the task of measuring the circumference of the earth, is supposed to have made use of very much the same kind of implements as those employed by modern astronomers. He must have had a very fair instrument for measuring angles, and have known very well how to use it, seeing he was able to determine a value for the obliquity of the ecliptic which agrees so well with that established by modern science, its variations being, for what we know, taken into account; and for length or distance he would doubtless have some implement analogous to the metre, chain, foot-rule, or something called by other name that would, in those days, present facilities for selling a yard of calico. His operations would probably be as plain and simple as those applied to the measuring of a village green—for we are not told that he had any idea of there being any difference between the length of a degree of the meridian at the equator and one nearer either of the poles—and involved no hypotheses or theories, any more than modern operations have done.

When the time came for making efforts to ascertain the density of the earth, science seems to have employed the very simplest means it had at its disposal for attaining its object, and to have gone on refining its implements and operations in conformity with the lessons it went on learning while pursuing its self-imposed task. Every one who, even for recreation, has read a fair amount of the multitude of works and writings that have been published on Popular Astronomy—not to speak of text-books—knows that the first attempts were made by measuring the attraction of steep, or precipitous, mountains for plummets suspended in appropriate positions in their neighbourhood; then—evidently from knowledge acquired during these operations—by the attraction for each other of large and small leaden balls suspended on frames and torsion balances, which go under the name of the Cavendish Experiment; and afterwards by a refinement on this in using the Chemical Balance, where only one large and one small ball of metal are required. All these operations and their results are to be found described in works of various kinds, and are generally reduced to something like the following tubular form, which we reproduce in order to make more intelligible what we have just said, and that we may make a few remarks upon them.

There is no hypothesis, no theory, connected with any of the operations, unless it was the supposition that a plummet—which was naturally believed to point to the centre of the earth—should be pulled to one side by the attraction for it of a mountain in its neighbourhood, and that was found to be a fact.

METHODS EMPLOYED FOR FINDING THE DENSITY OF THE EARTH, AND THEIR RESULTS.

Experiments made.	By whom, and Date.		Mean Density found.
(1) *Deviation of Plummet by the Attraction of Mountains*:—			
At Schiehallien	Maskelyne	1772	4·713
At Arthur's Seat	Sir H. James	1855	5·316
(2) *Torsion Balance Experiments*:—			
	Cavendish	1798	5·448
At Freyberg, Saxony	Reich	1837	5·438
At Manchester	Francis Baily	1838–1842	5·675
(3) *Chemical Balance Experiments*:—			
	J. H. Pointing	1878	5·690

In the case of the plummet deviating from its absolutely straight direction towards the centre of the earth, caused by their attraction, not only the mountains themselves had to be measured and virtually weighed as far as they were measurable, but the weight of the wedge or pyramid between that measurable point, in each case, and the centre of the earth had to be estimated in some way; then the centre of gravity of the whole of this mass had to be ascertained, as well as the respective distances from the centre of the earth of this centre of gravity and that of the plummet, and only after all this and a deep study of the mutual attractions of this mass and the plummet could an estimate be formed of the mass of the earth. It will thus be seen that such measurements and estimates could never be looked upon as very exact and reliable; and nevertheless they have come very near the density of 5·66 finally adopted for the earth.

In the case of the Torsion Balance experiments a very considerable advance was made in consequence, most undoubtedly, of the knowledge acquired from what had been done by Maskelyne. When it was found that the attraction of Schiehallien for the plummets was such a measurable quantity, Cavendish evidently saw that the attraction of manageable leaden balls for each other would be measurable also, and that as no calculations of any kind whatever were necessary to find the masses of the balls, the mutual attraction of large and small balls would furnish a more exact means of measuring the density of the earth, than the roundabout way of having to calculate the weight of a mountain as a beginning; and with the requisite ingenuity, invention, and labour, he found the means of applying the torsion balance, to make the experiments.

After these experiments were revised by Reich and Baily —and the density of 5·66 adopted, we believe—still another set were undertaken by J. H. Pointing, with the Chemical Balance, in which only two metal balls, one large and one small were required, which gave a density of 5·690 as shown opposite, and from its extreme simplicity may perhaps have been the most exact of all.

We have said, we think with truth, that there is no hypothesis or theory involved in any of these experiments, but only the simplest form of—we might almost say—arithmetical calculation. But there is a theory built up on hypothesis which has no foundation whatever, and about which most people, who take the trouble to study it out to the very end, will come to the conclusion that "the less said the better." This, at all events, is our opinion, and we would not have taken any notice whatever of it had it not been that up to the present day, it is published in many works on Popular Astronomy, and even in some text-books, and is looked upon in them, apparently, as an example of the transcendent height to which human science can reach.

We allude, of course, to the theory that the deeper we go down into the earth—at least to an undefined and undefinable depth—the greater is its attraction for the bob of a pendulum at that depth, and the greater the number of vibrations the pendulum is caused to make in a given time. The explanation of the theory is, that were the earth homogeneous throughout its whole volume, the pendulum ought to make the fewer vibrations, the deeper down in the earth it is placed; but as the earth is not homogeneous, it actually makes a greater number of vibrations in a given time, because the attractive force of the earth increases—up to the undefined and undefinable depth—on account of the denser matter beneath the pendulum bob more than overbalancing the loss of attraction from the lighter matter left above it. The author of the theory was the late Astronomer Royal, Sir George B. Airy, who from it endeavoured to calculate the mean density of the earth, and with that view made two experiments which are thus described by Professor C. Piazzi Smythe in his work on the Great Pyramid:—

"Another species of experiment . . . was tried in 1826 by Mr. (now Sir) George B. Airy, Astronomer Royal, Dr. Whewell, and the Rev. Richard Sheepshanks, by means of pendulum observations at the top and bottom of a deep mine in Cornwall; but the proceedings at that time failed. Subsequently, in 1855, the case was taken up again by Sir

George B. Airy and his Greenwich assistants, in a mine near Newcastle. They were reinforced by the new invention of sympathetic electric control between clocks at the top and bottom of a mine, and had much better, though still unexpectedly large results—the mean density of the earth coming out, for them, 6·565."

From other sources we have also found that the pit, or mine, was at the Harton Colliery and 1260 feet deep, that the pendulum at the bottom of it gained $2\frac{1}{4}$ seconds on the similar one at the top, in 24 hours; and that the surrounding country had to be extensively surveyed, the strata had to be studied, and their specific gravities ascertained.

A little unbiassed thought bestowed on this theory will at once show that it begins by violating the law of attraction discovered by Newton, when he showed *that the mutually attractive forces of several bodies are the same as if they were resident in the centres of gravity of the bodies.* In the case in point this means, that the attraction of the earth for the bob of the pendulum at the top of the mine was the same as if all its force was collected at its (the earth's) centre. In that position the force of the earth's attraction comprehended, most undeniably, the whole of its attractive power, including whatever might be imagined to be derived from the non-homogeneity of the earth, due to its density increasing towards the centre; and we are called upon to believe that when, virtually, the same pendulum was removed to the bottom of the mine, and a segment 1260 feet thick, at the centre as good as cut off from the earth and—as far as the pendulum was concerned—hung up on a peg in a laboratory, the diminished quantity of its matter had a greater attractive force, a very little beyond the centre—non-homogeneity again included—than the whole when the sphere was intact. This we cannot do, because all that we can see in the placing of the pendulum at the bottom of the mine, is that the position of the bob has divided the earth into two sections, one of which has a tendency to pull it up towards the surface, and the other to pull it down towards its centre of gravity; and because the mass of the smaller segment is so insignificant that its entire removal to the laboratory peg, not only could not produce the reverse action, on which the theory is based, but could not be measured by any stretch of human invention or ingenuity; it is far beyond the reach of mathematics and human comprehension of quantity.

The difficulty of belief is increased when we reflect that, were the pendulum taken down towards the centre of the earth, the number of its vibrations in a given time ought gradually to decrease as it approached the centre, and would cease altogether when that point was reached. And we feel confident that no mathematician could calculate where the theoretical acceleration of the vibrations would cease, and the inevitable retardation commence; where the theory would come to an end and the law of attraction begin to assert its rights, simply because he does not know how the non-homogeneity is distributed in the earth. No man can tell, even yet, how the mean density of 5·66 is made up throughout the earth, and without that any theory founded on its non-homogeneity is out of place.

But to follow up our assertion of non-commensurability. Taking the diameter of the earth at 8000 miles, and its mean specific gravity at 5·66, its mass would be represented by 1,517,391,000,000 cubic miles of water. On the other hand, supposing the earth to be a true sphere, the volume of a segment of it cut off from one side, at one quarter of a mile deep—not 1260, but 1320 feet—would be 785·35 cubic miles in volume, and if we suppose its specific gravity to be 2·5—greater most probably than the average of all the strata in the neighbourhood of the Harton Colliery—its mass would be represented by 1963·38 cubic miles of water. Then, if we divide the mass of the section below the pendulum, that is, 1,517,391.000,000 minus the mass of the one above it, 1963·38, viz. 1,517,390,998,036·62 by the mass of 1963·38 just mentioned, we find that the proportion they bear to each other is as 1 to 772,846.315. This being so, we are asked to believe that by removing $\frac{1}{772.846.315}$th part of the mass of the earth from one side of it, its force of attraction at the centre will not only not be decreased, but will be so increased that it will cause a pendulum, suspended at the centre of the flat left by the removal of the segment, to vibrate 86,402·25 times in twenty-four hours instead of 86,400 times as it did when suspended at the surface before the segment was removed; that is, that the vibrations will be increased by $\frac{1}{38,400}$th part. Again we cannot do so. Had we been asked to believe that the removal of so small a fraction as $\frac{1}{772,846,315}$th had decreased the earth's attraction at its centre, so much as to produce a diminution of $\frac{1}{38,400}$th part in the number of vibrations of the pendulum, we could not have done so; how much less then can we believe that the central attractive force had increased so much as to produce an augmentation of the vibrations in the same proportions? But more in this strain presently.

We have no doubt whatever that Sir George B. Airy and his assistants satisfied themselves that the pendulum at the bottom of the mine gained $2\frac{1}{4}$ seconds in twenty-four hours over the one at the top, but they may have been deceived by their over-enthusiastic adoption of what seemed to be a very grandly scientific theory, or by some unperceived changes in the temperature in the pendulums, caused by varying ventilation in the mine or the varying weather outside of it, or by the insidious manifestations of the "sympathetic electric control between clocks at the top and bottom of a mine," called in to assist at the experiments. An error of $\frac{1}{38,400}$th part of the time the sympathetic electricity would take to travel from the top to the bottom of the shaft would be sufficient to make the experiments of no value whatever; not to speak of the small errors that may have been made in surveying the surrounding country, calculating the specific gravities of the strata—for we are told that all this had to be done—and applying the elements thus obtained to the solution of the problem they had in hand. We have read of the difficulties met with by Mr. Francis Baily when he began to revise the Cavendish Experiment—some twelve or fifteen years before the final Harton Colliery experiments were made, and suppose it possible that they met with similar difficulties without being aware of it. And $\frac{1}{38,400}$th part is such a very small fractional difference in the vibrations in twenty-four hours, of the pendulums of the two separate clocks, that—taking into consideration the circumstances under which it was found—it would hardly be looked upon as reliable at the present day, when the clocks of astronomical observatories are placed in the deepest cellars or even caves available, so as to free them as much as possible from variations of temperature.

Having referred to the difficulties met with by Mr. Baily, we believe it worth while to transcribe Professor C. Piazzi Smythe's account of them, given in his work already referred to at page 22; because it not only has a very direct bearing on what we have been saying of changes of temperature, but is exceedingly interesting, and probably very rarely to be met with in other works. It is as follows:—

"Nearly forty years after Cavendish's great work, his experiment was repeated by Professor Reich of Freyberg, in Saxony, with a result of 5·44; and then came the grander repetition of the late Mr. Francis Baily, representing therein the Royal Astronomical Society, and, in fact, the British Government and the British Nation.

"With exquisite care did that well-versed and methodical observer proceed to his task, and yet his observations did not prosper.

"Week after week, and month after month, unceasing measures were recorded; but only to show that some disturbing element was at work, overpowering the attraction of the larger on the smaller balls.

"What could it be?

"Professor Reich was applied to, and requested to state how he had continued to get the much greater degree of accordance with each other, that his published observations showed.

"'Ah!' he explained, 'he had to reject all his earlier observations until he had guarded against variations of *temperature* by putting the whole apparatus into a cellar, and only looking at it with a telescope through a small hole in the door.'

"Then it was remembered that a very similar plan had been adopted by Cavendish, who had furthermore left this note behind him for his successor's attention—'that even still or after all the precautions which he did take, minute variations and small changes of *temperature* between the large and small balls were the chief obstacles to full accuracy.'

"Mr. Baily therefore adopted yet further, and very peculiar, means to prevent sudden changes of temperature in his observing room, and then only did the anomalies vanish and the real observations begin.

"The full history of them, and all the particulars of every numerical entry, and the whole of the steps of calculation, are to be found in the Memoirs of the Royal Astronomical Society, and constitute one of the most interesting volumes (the Fourteenth) of that important series; and its final result for the earth's mean density was announced as 5·675, probable error $\pm$ 0·0038."

After reading this story of Baily's experiments with care, one cannot help feeling something stronger than want of confidence in those made at the Harton Colliery, especially after what has been shown of the smallness of the fraction of the earth that was dealt with, and due consideration is given to the insignificant difference of effect that the non-homogeneity of the earth could produce on the remainder after the supposed removal of such a small fraction; and here we might let the theory drop. Perhaps it may be thought that now there is nothing to be gained by spending time and work in showing it to be more truly erroneous than we have yet made it out to be; but if there is error, it cannot be too clearly exposed, and the sooner it is put an end to, the better; more especially as it has been accepted as true by some authors of text-books, and by some competent astronomers who, in trying to explain the anomaly of the increase instead of decrease in the force of attraction at the bottom of a mine compared with the top, have used arguments which are not consistent with the law of gravitation, or rather attraction.

Messrs. Newcomb and Holden in their work, entitled "Astronomy for High Schools and Colleges," sixth edition, 1889, apparently accept the theory, and proceed to explain and support it by showing what would be the action of a hollow spherical shell of any substance on a particle of it, say the bob of a pendulum, placed on the outside and also on the inside of the shell; and give us two theorems which are supposed to comprehend both cases. These are:—

(1) "If the particle be outside of the shell, it will be attracted as if the whole mass of the shell were concentrated at its centre."

(2) "If it be inside the shell, the opposite attractions in every direction will neutralise each other, no matter whereabouts in the interior the particles may be, and the resultant attraction of the shell will therefore be zero."

To the first theorem no objection can be made: The particle on the outside of the shell will undoubtedly be attracted by every particle in the shell, with the same force as if the attractive power of all the particles composing it were concentrated in the centre. Not so with the second theorem: for it can be objected that it altogether ignores the Law of Attraction laid down by Sir Isaac Newton, where it asserts that the resultant attraction of the shell for the particle will be zero, when it is placed anywhere on the inside. In fact the theorem supposes a case impossible for the Harton Colliery experiments, in order to demonstrate their accuracy; for it makes use of the bob of the pendulum—a particle of matter—as if it were transferable to any part of the interior of the earth instead of being confined within the bounds of its swing. That the attraction of the shell—1260 feet thick all round the earth—on the pendulum bob inside of it continues in all its force, and is only divided into two opposing parts, is made plain by Fig. 1. Supposing O to represent the bob of the pendulum at the bottom of the mine, and the space between the two circles the shell of the earth. Then the line B C will show where the attraction of the shell for the bob is divided into two parts acting in opposite directions. Supposing these two parts to be separated from each other, only far enough to admit the bob—a particle to all intents and purposes—between them; the part B A C will attract the bob as if its whole attractive force were collected at its centre of gravity, and the part B D C as if the whole of its attractive force were collected, not at the centre B of the shell, but at its centre of gravity, a very little distance from B in the direction towards D. This is an incontrovertible fact, because it is in strict accordance with Newton's Law of Attraction, which is : *Every particle of matter in the universe attracts every other particle with a force directly as their masses, and inversely as the square of the distance which separates them.*

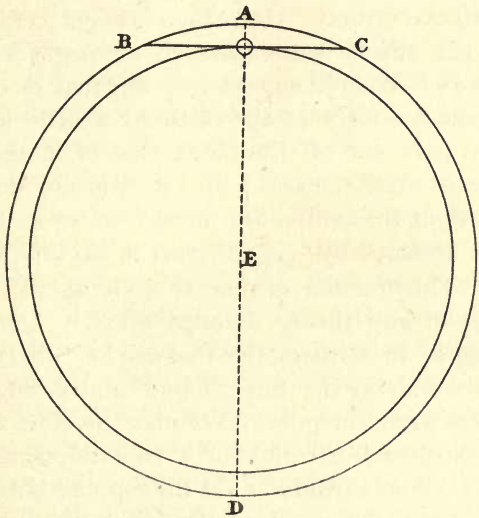

FIG. 1.

If we now suppose the interior of the shell to be filled up solid, that will make no difference, because the mass of the part B D C will only be increased vastly thereby, while the mass of A B C will remain the same ; the two parts only increasing their proportion to each other, and thus coming to be for the earth—in the Harton Colliery experiments—what we represented them to be at page 24 ; and we can now proceed to find the attractive force of each of the two masses for the bob of the pendulum which is as the inverse square of their distances from it. These distances may be taken, without any very great stretch of conscience, as one-tenth of a mile and 3999·75 miles; because the centre of gravity of the segment A B C will be about that distance from O, and that of B D C cannot be adequately represented by a greater sum than 3999·75, always supposing the diameter of the earth to be 8000 miles. Thus the squares of these two distances will be 0·01 and 15,898,000 miles respectively, and the relative force of attraction for the pendulum of the two segments A B C and B D C will be as $1 \times 0 \cdot 01$ and 772,846,315, and $772,846,315 \times 15,898,000$; that is as 1 is to 1,228,671,000,000,000,000. Here then we get confirmed the unbelief in the theory we expressed at pages 23 and 24. Surely no one will be bold enough to assert that by decreasing the total attractive force of the earth by a little less than a $1\frac{1}{4}$ trillionth part cut off from one side of it, the want of homogeneity in what remains will not only not decrease its attractive force at the centre, but increase it so as to make a pendulum be lessened by $\frac{1}{38,400}$th part of its time in beating one second. This fraction of time is quite small enough to inspire doubt of any theory founded upon it; and if there ever is a quantity in mathematics that can be called negligible, the fraction of attractive force found above ought to be included in the same category. We may therefore assert that no human measurements could find a true difference between the beats of a seconds pendulum at the top and bottom of the pit at the Harton Colliery. If all the people who have puzzled themselves with this theory had spent an hour or two in making the above calculations before they began them, there would have been no experiments made, and the theory would have died almost ere it was born. Those who believed in it may have looked upon a particle as a negligible quantity, but as the whole earth is made up of particles a little thought would have put an end to such a notion. What puzzles us is how such a theory could be formed by people who knew nothing whatever of the nature of the interior of the earth at a depth of even one mile, and how they could speculate on its want of homogeneity without knowing anything of how the density of 5·66 is made up in it? To suppose that the earth is made up of strata of different densities, and that each is in some degree elliptical—the ellipticity of one stratum being different from another, as the French mathematician Clairaut did—is all very allowable; but to build up any theory on any such suppositions is to build upon shifting sands without examining the foundations. For anything that is known up to the present time, the density of the earth may go on increasing gradually from the surface to the centre, or it may attain nearly its greatest density at a few miles from the surface, and continue homogeneous or nearly so from there to the centre.

To go further now: it is not true that the attraction of a hollow shell of a sphere for any particle within it, is the same "no matter whereabouts in the interior the particle may be." The only place where the attraction will be the same is when the particle is at the centre. In that position a particle would be in a state of very unstable equilibrium, and a little greater thickness of the shell on one side than the others, would pull it a little, perhaps a great, distance from the centre towards that side; and if we extend our ideas to a plurality of particles within the shell of a sphere, we are led to speculate on how they would be distributed, and to see the possibility of there not being any at all at the centre. This is a point which has never been mooted, as far as we have been able to learn, and we shall have to return to it when the proper time comes.

It is difficult to understand how any man could conceive the notion that a shell of a sphere, such as that shown at Fig. 1, could have no attraction for each separate one of all the particles which make up the mass of the whole solid sphere within it; for that is the truth of the matter if properly looked into, when it is asserted, as has been done by Messrs. Newcomb and Holden, that "the resultant attraction of the shell will therefore be zero." If such a notion could be carried out in a supposed formation of the earth, an infinity of particles would carry off the whole of the interior, and leave the earth as only a shell of 1260 feet thick, as per the Hartley Colliery experiment; only we are told, or left to understand, that that process could not go on for ever, but would have to come to an end somehow and somewhere; and then we are left to speculate on how the unattracted particles could come back to take part in the composition of the earth. Left to ourselves we can only liken the process to that followed by a man who peels off the outer layer of an onion, eats the interior part, and when he is satisfied throws down the outer layer and thinks no more of it ; not even that he might be asked what had become of the interior part.

Curiously enough, there is a way of explaining how, or rather why, the notion was formed—not unlike the one just given—to be found in the third of Sir George B. Airy's lectures on Popular Astronomy, delivered at Ipswich several years before the final experiments were made at the Harton Colliery. In that lecture, while describing how the Greek Astronomers accounted for the motions of the sun and planets round the stationary earth, he says, "It does appear strange that any reasonable man could entertain such a theory as this. It is, however, certain that they did entertain such a notion ; and there is one thing which seems to me to give something of a clue to it. In speaking today and yesterday of the faults of education, I said that we take things for granted without evidence ; mankind in general adopts things instilled into them in early youth as truths, without sufficient examination ; and I now add that philosophers are much influenced by the common belief of the common people."

We can agree with Sir George B. Airy in his ideas about education, and now conclude by saying that he has given us a very clear and notable example of a theory being accepted very generally, without being thoroughly examined to the very end, and of how easy it is for such theories to be handed down to future generations for their admiration.

CHAPTER II.

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48 Want of proper study gives rise to extravagant conceptions, jumping at conclusions, and formation of "curious theories."

A GOOD deal of theorising has been expended in accounting for the absence of all but traces of an atmosphere and water on the moon, which might have been avoided had astronomers not caught up the notion, and stuck to it, that it rotates on its axis once for every revolution that it makes round the earth. It might be difficult to find out with whom the notion originated; but perhaps it was first conceived to be the case by some celebrated astronomer, and has been accepted by almost all his successors without being properly looked into. Any one who chose to take the trouble to study the matter thoroughly, would have easily discovered that the moon can have no rotation of any kind on its axis, and immediately afterwards have found out the reason why nothing beyond traces of air and water were to be seen on the side of it constantly turned towards the earth. This is another example we can give of erroneous ideas leading to erroneous and impossible conclusions, and preventing the truth from being discovered. That the rotation of the moon on its axis is stated to be a fact, by recognised and celebrated astronomers, will be seen from the following quotations.

(1) Sir John Herschel, in his "Treatise on Astronomy," new edition of 1835, says at page 230: "The lunar summer and winter arise, in fact, from the rotation of the moon on its own axis, the period of which rotation is exactly equal to its sidereal revolution about the earth, and is performed in a plane 1° 31′ 11″ inclined to the ecliptic, and therefore nearly coincident with her own orbit. This is the cause why we always see the same face of the moon, and have no knowledge of the other side."

(2) In his "Poetry of Astronomy," page 187, Mr. Proctor says: "For my own part, though I cannot doubt that the substance of the moon once formed a ring around the earth, I think there is good reason for believing that when the earth's vaporous mass, receding, left the moon's mass behind, this mass must have been already gathered up into a single vaporous globe. My chief reason for thinking this is, that I cannot on any other supposition find a sufficient explanation of one of the most singular characteristics of our satellite—her revolution on her axis in the same mean time, exactly, as she circuits around the earth."

(3) Professor Newcomb, in his "Popular Astronomy," 5th edition, 1884, at page 313, has what follows: "The most remarkable feature in the motion of the moon is, that she makes one revolution on her axis in the same time that she revolves around the earth, and so always presents the same face to us. In consequence, the other side of the moon must remain for ever invisible to human eyes. The reason for this peculiarity is to be found in the ellipticity of her globe." Then he enlarges upon and confirms the fact of her rotation.

(4) Mr. George F. Chambers, in his "Handbook of Astronomy," 4th edition, 1889, says at page 119, Vol. I.: "In

order that the same hemisphere should be continually turned towards us, it would be necessary not only that the time of the moon's rotation on its axis should be precisely equal to the time of the revolution in its orbit, but that the angular velocity in its orbit should, in every part of its course, exactly equal its angular velocity on its axis."

It may be necessary, to avoid misconception, to note that angular velocity on its axis confirms rotation; and what is more extraordinary, that Chambers must have thought that its angular velocity on its axis must have increased and diminished in order to agree with its increased and diminished velocities in its elliptic orbit at its perigee, apogee, and quadratures. A rather strange notion in mechanics where there is no provision made for acceleration or retardation of rotation.

(5) Dr. Samuel Kinns, in "Moses and Geology," twelfth thousand, 1889, says at page 208, "the same side of its (the moon's) sphere is always towards us. This could only happen by its having an axial rotation equal in period to its orbital revolution, which is 27 d. 7 h. 43 m. 11 s.

(6) In the "Story of the Heavens," Sir Robert S. Ball informs us, in the fifteenth thousand, 1890, page 530, "That the moon should bend the same face to the earth depends immediately on the condition that the moon should rotate on its axis inprecisely the same period as that which it requires to revolve around the earth. The tides are a regulating power of the most unremitting efficiency to ensure that this condition should be observed."

(7) And finally we have what follows from Messrs. Newcomb and Holden, at page 164 of their work already referred to at page 27, "The moon rotates on her axis in the same time and in the same direction in which she moves around the earth. In consequence, she always presents very nearly the same face to the earth." And in a footnote to this consequence, add: "This conclusion is often a *pons asinorum* to some who conceive that, if the same face of the moon is always presented to the earth, she cannot rotate at all. The difficulty arises from a misunderstanding of the difference between a relative and an absolute rotation. It is true that she does not rotate relatively

to a line drawn from the earth to her centre, but she must rotate relative to a fixed line, or a line drawn to a fixed star."

In six of the above cases it is distinctly maintained that the moon rotates once on its axis in the same time that it makes one revolution round the earth, and that it is in consequence of this rotation that it always presents the same side to the earth. Thus we feel authorised to conclude that their authors did either believe that it does so rotate, or that they entertained some confused idea on the subject, which they did not take the trouble to examine properly, but accepted as a dogma, because some predecessor, with a great name, had stated that such rotation was necessary in order that its same side should be always turned towards the earth. In the seventh case the authors, while actually making the same assertion, try to persuade those who they acknowledge can see that the moon does not rotate on its axis in any sense, that their difficulty in comprehending what is meant by rotation, arises from the misunderstanding of the difference between an absolute rotation and one relative to a line drawn to a fixed star. But they do not attempt to show how this relative rotation has anything to do with or has any effect in causing the moon to present always the same side to the earth ; and leave the story in the same confused state, out of which nobody can draw any satisfactory conclusion. Also, though they distinctly recognise that it does not rotate relatively to a line drawn from the surface of the earth to its centre, they do not include in their general description of the moon anything in any way connected with what would be the consequences of its not really rotating on its axis relatively to the earth. So they leave us the problem in much the same state as they found it, and it is still necessary to show that there can be no actual rotation of any kind on its axis ; and the worst of it is that it is a thing that will have to be done in such very plain language that it will compel people to think of the absurdity of the idea so generally accepted.

To begin, it is very difficult to comprehend what the authors, above alluded to, meant by saying that the moon "must rotate relative to a fixed line, or a line drawn to a fixed star." It may mean relative to the line itself or to the star to which it is drawn. If it is to the line itself we cannot form any notion of what direction the rotation will have, direct, retrograde, or otherwise ; and if it is relative to the star itself, then we can see that the relative rotation must depend on what is the position of the star. Should it be placed in the " milky way," we can understand how the moon could show every side it has—almost, not quite—to the star during every revolution it makes round the earth, and how they may look upon it as a relative rotation. But if we draw the line to the pole star we cannot see how the moon can show every side it has to it in every revolution round the earth, so there can be no relative rotation in that case—and the " almost, not quite," applies to every star between the pole and the ecliptic. The moon shows only the northern hemisphere, or a little more due to libration of its own kind, to that star, and would have to remove its poles to the equator, and make a new departure, in order to show the whole of its surface to that star in every revolution round the earth. Thus it is clear that the explanation given us of the relative rotation, is evidently one of the kind not properly thought out to the end.

No one has ever said, or perhaps even thought, that a gin-horse makes one rotation on his vertical axis, in the same time as he makes a circuit round his ring, but, all the same, he keeps his same side always towards the gin, or mill, he is giving motion to. The proof that he does not make any such rotation is easy—no proof is really required. But, suppose he is giving motion to a whim for raising ores from a mine, and that his motion is what is called direct. When the cage containing the ore is brought to bank, is emptied, and has to be lowered into the mine again, the horse has then to reverse his motion to retrograde, in doing which he has to make a half rotation on his vertical axis, and turn his other side to the whim. When again the cage has to be raised to bank, he has to resume his direct motion, for which he has to make another half rotation on his vertical axis, but it is this time in the opposite direction. Thus it is shown that he can only make half rotations, under any circumstances, on his axis, and these in opposite directions, when he changes his motion from direct to retrograde, or *vice versâ* ; and that, when he moves in only one direction he cannot make even one rotation on his vertical axis, however long he may travel round the mill. In the same manner the moon which never turns back in its orbit can never make even one half rotation on its axis, which is all that we have had to prove. It is hardly necessary to observe that its axis is nearly parallel to the earth's, just the same as the horse's is to that of the whim. Neither could any one say that the relative rotation of the horse to a star, or tower, or, say, a bridge, outside of his ring, could have any effect on his revolution round the mill, or his always keeping his same side to it, there being no mechanical connection between them, nor any law of attraction ; and the same is the case between the moon and a fixed star.

Now, we may begin to consider what effects must be produced by the moon not rotating on its axis, and we can do so most easily by continuing to work with our gin horse, or some equivalent substitute. It would not cost a great deal of ingenuity to plant a steam engine in the centre of the mill he is supposed to be driving, and to drive with it not only the mill but the horse also at the end of his lever. There might be some dissipation—Professor Tate would call it degradation —of energy in such an experiment, but we could get over that by making *divina Palladis arte* a wooden horse. We might arrange the steam-engine so as to cause the mill to make $27\frac{1}{3}$ revolutions for one made by our wooden horse, and so have a sort of a model of the earth and moon performing their most important relative motions. Then, having got our model ready for action, instead of filling it *armato milite* we might fill it half full of water. We fill it only half full, because the armed soldiers could not lie on the top of each other in the *other* horse, and there would be a vacant space above them for air, thus making the resemblance between the two the more similar ; and also because it suits our purpose better, as will soon be seen. We have still to propose that a lot of holes should be supposed to be made in the sides of *our* horse all round, just a little higher than between wind and water.

Pallas did not order any holes to be made in *hers* as far as we know, even for ventilation, though we think it would have been an advantage ; but that will not spoil the experiment we are now prepared for. Let the steam-engine be started now and we shall soon see what will happen to the water. As the speed increases it will not be long till it begins to be thrown out, not from the side turned towards the mill but from the one furthest from it ; and if it is increased sufficiently the whole of it will be very soon thrown out. If we could now close up the holes on the side of the horse turned towards the mill, it would so happen that a good deal of the air would be expelled also ; and if the speed of the horse were brought up so as to equal that of the moon in its orbit, there would be nothing more, at the most, than traces of air left even in it. The expelling agent in this experiment would, of course, be centrifugal force, and we do not need to exercise our mental faculties very greatly, to comprehend that it is the same force that has driven both air and water away from the side of the moon always turned towards the earth. All the difficulty we have to contend with will be to make sure that the orbital velocity of the moon is sufficient to produce the force required. That the force is exceedingly greater than what is required is proved by the fact, that the velocity with which the moon travels in its orbit is a little more than 38 miles per minute, whereas the velocity of the circumference of a centrifugal machine, used for clarifying sugar, drying clothes, or any other similar industrial purpose, does not require a greater velocity than about *one* mile per minute, in order to throw everything in the form of water out of the material to be dried, and out of the centrifugal machine itself; and we know that air would be expelled more easily than water, were none re-admitted to supply the place of what was expelled.

Here the idea very naturally occurs to any one, that so great a velocity would drive both air and water away, even from the far off side of the moon, into space, but in order to do so the velocity would have to be 120, not 38, miles per minute. Our authority for this statement will be found in "The Nineteenth Century," for August 1896, in an article written by Prince Kropotkin, in which he says: "But it appears from Dr. Johnstone Stoney's investigations that even if the moon was surrounded at some time of its existence with a gaseous envelope consisting of oxygen, nitrogen and water vapour, it would not have retained much of it. The gases, as is known, consist of molecules rushing in all directions at immense speeds; and the moment that the speed of a molecule which moves near the outer boundary of the atmosphere exceeds a certain limit (which would be about 10,600 feet in a second for the moon) it can escape from the sphere of attraction of the planet. Molecule by molecule the gas must wander off into interplanetary space; and the smaller the mass of the molecule of a given gas, the feebler the planet's attraction, and this is why no free hydrogen could be retained in the earth's atmosphere, and why the moon could retain no air or water vapour."

A velocity of 10,600 feet per second is as near 120 miles per minute as there is any use for, which is more than three times as great as the velocity of the moon in its orbit, so there is no possibility whatever of air and water having been swept away from the far off side of it by centrifugal force; more especially as it ought to be well known that that force is always counteracted by the attractive force of the satellite for these or any other elements.

We do not want to discuss the point of whether the mutual collisions of the molecules of a gas could get up such a velocity as would enable them to free themselves from the attraction of the moon, for it looks to us too much like one of those notions that are got up to account for something that does not exist; but we do want to state our dissent to the conclusion—evidently jumped at—that because there are hardly any signs of there being air or water on our side of the moon, there can be none on the other. No astronomer, physicist, scientist of any kind, can prove that there is none, simply because he has never been round there to see or make experiments to prove it; and if there is any one bold enough to make such an assertion, it is only an example of how stupendous a jump to a conclusion can be made.

When we first read, many years ago, some of the reasons given for there being no water visible on the side of the moon constantly turned to the earth, one of which was that if there ever had been any it must have been absorbed into its body during the process of cooling and consolidation; and when we had convinced ourselves, by placing two oranges on two ends of a wire and revolving the one round the other, that the moon did not rotate on its axis in any sense whatever, we came to the conclusion that both water and air could be removed to the far off hemisphere by centrifugal force. We thought this so simple, so self-evident, and so indisputable an explanation, that every one who had read what we had read must have come to the same conclusion; so that we were not a little surprised when we saw it stated by "The Times" of September 15, 1893, in its first report of the meeting of the British Association for that year, that Sir Robert Ball had suggested, some time previously, that the "absence of any atmosphere investing the moon is a simple and necessary consequence of the kinetic theory of gases." This at once made us suspect that the theory—our theory—must have been new, but we could not altogether believe it. It seemed to us passing strange that it should not have occurred to astronomers, from the moment they discovered that they could not find any, or hardly any, traces of air or water on the only hemisphere they could examine; but it would appear from Sir Robert Ball's suggestion, being even discussed at that meeting, that the notion of their having been removed simply by centrifugal force to the unseen hemisphere, had never been entertained by, to say the least, any one who was present at that discussion.

Not satisfied with this conclusion, we proceeded to examine all the books, journals, magazines, and *papers* we could get hold of, to see whether we could find any indication of such a conception having been published previously, and the nearest approach to anything of the kind having been conceived of by anyone, we found in Chambers's work—already referred to —at page 134, Vol. I., where we read, "Professor Hansen has recently started a curious theory from which he concludes that the hemisphere of the moon which is turned away from the earth may possess an atmosphere. Having discovered certain irregularities in the moon's motion, which he was unable to reconcile with theory, he was led to suspect that they might arise from the centre of gravity of the moon not coinciding with the centre of figure. Pursuing this idea, he found upon actual investigation that the irregularities could be almost wholly accounted for by supposing the centre of gravity to be at a distance of 33½ miles *beyond* the centre of figure. Assuming this hypothesis to be well founded, Professor Hansen remarks that the hemisphere of the moon, which is turned towards the earth, is in the condition of a high mountain, and that consequently we need not be surprised that (little or) no trace of an atmosphere exists; but that on the opposite hemisphere, the surface of which is situated *beneath* the mean level, we have no reason to suppose that there may not exist an atmosphere and consequently both animal and vegetable life. Professor Newcomb has disputed these conclusions of Hansen, which it is obvious must be very difficult of either proof or disproof."

What Professor Newcomb's objections to the conclusions of Hansen were we do not know, but we do know that Mr. Proctor also objected to the "curious theory," as it is called by Mr. Chambers. In his "Poetry on Astronomy," he discusses pretty fully the withdrawal of water from the surface of the moon during the process of cooling and condensation, ascribing the conception of it to four independent authors, namely, Seeman, a German geologist, Frankland in England, Stanislas Mennier in France, and Sterry Hunt in America; and in a footnote, at page 163, says of Hansen's theory: "The idea was that the moon, though nearly spherical, is sometimes egg-shaped, the smaller end of the egg-shaped figure being directed towards the earth. Now, while it is perfectly clear that on this supposition the greater part of the moon's visible half would be of the nature of a gigantic elevation above the mean level, and would, therefore, be denuded (or might be denuded) of its seas and denser parts of the air covering it, yet it is equally clear that all around the base of this monstrous lunar elevation, the seas would be gathered together, and the air would be at its densest. But it is precisely round the base of this part of the moon or, in other words, round the border of the lunar hemisphere, that we should have the best chance of perceiving the effects of air and seas, if any really existed; and it is because of the absolute absence of all evidence of the kind, that astronomers regard the moon as having no seas and very little air."

Had the idea of centrifugal force ever occurred to Mr. Proctor, he could not have written this last sentence; for he could not have failed to see that "the border of the visible lunar hemisphere" would be the very place, from which it could most easily remove air and water, after they had got so far down the monstrous elevation; because there it—the centrifugal force—would be acting at right angles to the moon's attraction, instead of having to contend against it, as it would have to do in a constantly increasing degree until it arrived at its maximum, just in porportion to the distance the air and water got down to the similar monstrous *depression* on the other hemisphere, down which the gradient would start off under the most favourable circumstances possible.

From what has been said, it is very evident that neither Hansen, Chambers, Proctor, nor any of those whose names have been mentioned by the last, in connexion with the withdrawal of water into the body of the moon by absorption, while cooling and condensing, had ever thought of the possibility of air and water having been removed by centrifugal force from the side of the moon turned towards the earth. That it should not have occurred to Hansen seems passing strange, seeing that he had conceived the idea of their possible existence on the hemisphere turned away from the earth, which could hardly fail to make him think of how they got there, and could exist only there; and the only explanation of his not having perceived the true cause seems to be, that his thoughts were hampered by a sort of confused notion that the moon actually rotates on its axis once for every revolution it makes around the earth, that being, as it were, one of the dogmas of astronomic belief, handed down from some great authority of times past, and never properly inquired into.

We do not want to question the suggestion, that the absence of any atmosphere investing the moon is a simple and necessary consequence of the kinetic theory of gases—though we see that a good deal could be argued against it—as we do not consider it to be necessary—neither the questioning nor the theory. We have demonstrated clearly, how both air and water could be removed from the side of the moon constantly shown to us, and that is sufficient for our purpose both now and later on; besides it would appear that the moon really has some sort of an atmosphere somewhere.

Following up the quotation, made at page 39, from Prince Kropotkin's article in the "Nineteenth Century" as being the latest information we have on the subject, we are told that "a feeble twilight is seen on our satellite, and twilight is due, as is known, to the reflection of light within the gaseous envelope; besides it has been remarked long since at Greenwich that the stars which are covered by the moon during its movements in its orbit remain visible for a couple of seconds longer than they ought to be visible if their rays were not slightly broken as they pass near the moon's surface. Consequently it was concluded that the moon must have an atmosphere" . . . and:

"The observations made at Lick, Paris, and Arequipa, fully confirm this view. A twilight is decidedly visible at the cusps of the crescent-moon, especially near the first and last quarters. It prolongs the cusps as a faint glow over the dark shadowed part, for a distance of about 70 miles (60″), and this indicates the existence of an atmosphere having on the surface of the moon the same density as our atmosphere has at a height of about forty miles."

What is of interest for us to know is where that "feeble twilight," or, "reflection of light within the gaseous envelope," is seen. Whether it is at what Mr. Proctor calls "the border of the visible lunar hemisphere," on this side of it, or beyond it. It cannot be a difficult matter to decide. It must be beyond it, for the following reasons: If the atmosphere has been driven away to the far-off hemisphere of the moon by centrifugal force, its natural tendency would be to spread out immediately after it had passed the visible border where we have said the centrifugal force would be acting most effectively. Also, if all the air at one time belonging to our side of the moon has been driven away to the other, that side must have a double allowance of atmosphere, which, though it does not increase its density at the surface, on account of the centrifugal force, will double its volume, and enable it to extend to a greater proportionate distance in all directions from the border and from the far-off hemisphere. In this way there must be a considerable wedge of atmosphere illuminated by the sun, and visible past the edge of the moon's disc, to reflect a feeble twilight—perhaps something stronger—towards the earth, and to intercept the light of a star before its edge and that of the moon come into actual apparent contact. But before the wedge becomes thick enough to reflect that light, the reflecting part must be far beyond the edge of the moon's disc. Perhaps the feeble light might be seen more clearly when looked for in the proper place; quite possibly hundreds of miles beyond the disc.

In order to make more clear the truth of what we have said about water and air—and more especially the latter—being thrown away to the far-off side of the moon by centrifugal force, we may add the following details: If the force of gravity at its surface is one-sixth part of what it is at the surface of the earth, the pressure of an atmosphere there would be 2·5 lb. per square inch, if it rotated on its axis; but as it does not so rotate and is subjected to centrifugal force, the pressure of an atmosphere will vary according to the part of it over which it exists. On the nearest part of the side turned towards the earth, gravity, which we have just seen must be equal to 2·5 lb., would be acting in the same direction as centrifugal force, which in its turn is equal to 0·7 lb. or thereby, and the whole would be 3·2 lb. per square inch tending to drive off air and water to the far-off hemisphere. But from that place, gravity would gradually diminish its aid till it came to be nil at the disc separating the two hemispheres, where it would have no effect whatever as it would be acting at right angles to centrifugal force, and this would be reduced to 0·7 lb. per square inch. Then, from the edges of the disc forward, on the far-off hemisphere, gravity would begin to act against centrifugal force, or rather *vice versâ*, until it, gravity, got reduced to 1·8 lb. per square inch. Also, as that hemisphere must have a double portion of air or atmosphere on it, and as its pressure on any part of it cannot be greater than the 1·8 lb. just mentioned, we can imagine that the double quantity will hang closer to the surface than if there was only one portion. Such being the case the atmosphere would spread out much more rapidly than would be represented by the extension of a triangle starting from the earth and reaching beyond the moon's disc to the farthest limit of the atmosphere ; and thus the wedge, which we have supposed to be visible beyond the edges of the disc may come to have a very considerable thickness. What that thickness may be, and up to what distance beyond the disc the density of the wedge would be sufficient to reflect the light of the sun, it would be very difficult to calculate, but we think it might possibly extend even as far as one-fourth of the radius of the moon—because at that point the force of gravity pulling it towards the centre, or the axis, would be very small, and its distance from the axis would be little less than the radius, not over 33 miles—and cause it to project over the edges as far, to appearance, as the 70 miles (60″) that have been observed at Greenwich. This reflected light must be all round the moon—not at the cusps only of the crescent-moon—and it has occurred to us that it may, most probably does, account for the appearance of what we call " the old moon in the young moon's arms." We know what effect the " earth-shine " has upon the moon at its change, and the brighter *ring-shine* just outside of it, may very well be caused by the sunlight reflected from the atmosphere far beyond the visible limit of the hemisphere turned to us.

In support of this suggestion we may refer to Professor C. A. Young's description, in his " Sun," p. 213, of one particular feature observed at the time of a total eclipse of the sun. He says :—" On such an occasion, if the sky is clear, the moon appears of almost inky darkness, with just a sufficient illumination at the edge of the disc to bring out its rotundity in a striking manner. It looks not like a flat screen, but like a huge black ball, as it really is. From behind it stream out on all sides radiant filaments, beams, and sheets of pearly light, which reach to a distance sometimes of several degrees from the solar surface, forming an irregular stellate halo, with the black globe of the moon in its apparent centre."

There can be little doubt, we think, from what is said here, that Professor Young looks upon this "illumination of the edge of the disc" as pertaining to the moon, and upon the "radiant filaments, beams," etc. behind it as belonging to the sun. And in that case the illumination can only be caused by the light of the sun, refracted by the atmosphere belonging to the hemisphere of the moon that is never seen from the earth.

We have taken it for granted in what we have been doing, that the moon has really rotated on its axis, and to some purpose, at some former period of its existence. Some people think otherwise, or that there is at least a doubt about it; we cannot see even the shadow of a doubt. All that we need to say in support of our opinion is, that there is no other conceivable way of accounting for its perfectly circular form. All the planets are circular, or spheroidal—to speak more correctly—in form, admittedly in consequence of rotation on their axes; and if one or two of Jupiter's satellites are not completely circular or spheroidal, it does not stretch our conscience very much to suppose that it is because they have not yet been rotated into form. Saturn apparently has satellites still in the form of rings, and there can be nothing out of the way in supposing that all of Jupiter's are not yet licked into shape. The fact that there is no appearance of compression on the moon makes us think of why there is none, and the only explanation that occurs to us is, that, as its rotation must have come to an end gradually, the compression it must have had when rotating must have disappeared gradually also, by reason of the differences of force in the equatorial and polar attractions, drawing in the bulged out, and thus forcing out the compressed parts. This is a notion that will be scoffed at by those who have always thought, and maintained, that the earth acquired its present form when in a liquid state ; but they have not thought this supposition—for it is nothing else—out to the very end. Several reasons could easily be given against their opinion, among others the variations in rate of rotation we so frequently see used in favour of other notions ; but we shall content ourselves with the best one of all, which is this : The pressures in the interior of the earth must be so enormous that they are quite sufficient to compress steel, or adamant if that is supposed to be more resistant, into any shape whatever, almost as if it were dough, and there can be no doubt—mathematics notwithstanding—that the earth has the form, to-day, due to its present rate of rotation. We shall have to return to this subject some time hence, if we live to complete what we have taken in hand.

How many things there are, in what is considered to be astronomical science, that have not been properly thought out to the end, and to what strange notions they have given rise! This one of the rotation of the moon which we have been discussing, has evidently given occasion for the conception of the theory that the absence of atmosphere and seas from the moon is the natural consequence of the kinetic theory of gases ; and the author of the theory, and its supporters, have never, apparently, taken the trouble to think whether their absence from the near hemisphere is a satisfactory and convincing proof of there not being any air or water on the far-off one. In what we have proposed to write many similar examples of want of study will be met with, but we do not intend to call special attention to them, unless it be in cases where we consider it to be of some importance to do so. In fact we have already been working on that plan.

CHAPTER III.

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WE have thought it worth while to dedicate this chapter to some remarks on cosmogonies in general, and examination into a very few conceived by eminent men; these forming in our opinion the most attractive matter for those readers who do not pretend to make a study of astromony, but are very desirous to have some knowledge of the most plausible ideas which have been conceived by astronomers, of how the universe, and more particularly the solar system, were brought into existence; while, at the same time, they are the subjects on which more crude conceptions, more limited study, and more fanciful unexamined thought have been expended, than any others we have met with. Some readers will, no doubt, be able to reject what is erroneous, to speak mildly, but there will be, equally surely, some who cannot do so; and it must be confessed there are a good many to whom the most complicated conceptions, and the most difficult of comprehension, are the most attractive.

A great many centuries ago, astromoners and philosophers had already conceived the idea that the sun and stars had been formed into spherical bodies by the condensation of celestial vapours; but when the telescope was invented, and the nature of nebulæ in some measure understood, it was not long till it came to be thought that the matter, out of which the sun and stars were formed, must have been much more substantial in its nature than celestial vapours. Being visible, they were naturally considered to be self-luminous, and consequently endowed with great heat, because the self-luminous sun was felt to be so endowed, though perhaps not with the same degree. Accordingly, astronomers began to form theories, or hypotheses, on the construction of the solar system out of a nebula, which, like everything else, went on each one improving on its predecessor as, through continued observation and study, more knowledge was acquired of the nature of nebulæ. The most notable of these cosmogonists were Descartes, Newton, Kant, and Laplace, each of whom contributed valuable contingents to the general work ; which may be said to have culminated about a century ago in the Nebular Hypothesis of the last-named ; for the many attempts that have been made to improve upon it, or to supplant it altogether, have been very far from successful.

The hypothesis is about a century old, as we have said, and there may still be many people who can remember having heard it denounced as a profane, impious, atheistic speculation, for it is not over half a century since the ban begun to be taken off it. Sir David Brewster, in his "Life of Newton," said of it, "That the nebular hypothesis, that dull and dangerous heresy of the age, is incompatible with the established laws of the material universe, and that an omnipotent arm was required to give the planets their positions and motions in space, and a presiding intelligence to assign to them the different functions they had to perform." With others, its chief defect was that the time required to form even the earth in the manner prescribed by it, must have been infinitely greater than six days of twenty-four hours each. In the meantime, geologists had also discovered that, for the formation of the strata of the earth, which they had been examining and studying, the time required for their being deposited must have been, not days of twenty-four hours, but periods of many millions of years each ; and the evidence adduced by them that such must have been the case was so overwhelming, that Theology had to acknowledge its force, and gradually to recognise that the days must have been periods of undefinable length. Thus relieved from the charge of heresy, the hypothesis rose rapidly into favour, and came to be generally accepted by the most eminent astronomers, subject always to certain modifications, which modifications have never been clearly defined, if at all. It was not, however, allowed to enjoy long the exalted station to which it had attained.

Astronomers had begun to consider from whence the sun had acquired the enormous quantity of heat it had been expending ever since the world began, and, after long discussion, had come to the conclusion that by far the greatest source must have been the condensation from the nebulous state of the matter of which it is composed. Having settled this point, it was calculated that the amount of heat derived from that and all other sources could not have kept up its expenditure, at the present rate of consumption, for more than twenty million years, and could not maintain it for more than from six to eight million years in the time to come. Owing in good part to this great difference between the calculations of astronomers and geologists about the age of the earth, the hypothesis began again to suffer in repute, and then all its faults and shortcomings were sought out and arrayed against it.

The chief defects attributed to it were: The retrograde motion of rotation of Uranus and Neptune and revolution of their satellites—that fault in the former having been noted by Sir John Herschel, in his Treatise on Astronomy already cited; the discovery of the satellites of Mars which exposed the facts, that the inner one revolves round the planet in less than one-third of the time that it ought to, and that the outer one is too small to have been thrown off by Mars, in accordance with the terms of the hypothesis; the exclusion from it of comets, some of which at least have been proved, in the most irrefutable manner, to form part of the solar system; and what can only be called *speculations*, on the formation of a lens-shaped nebula brought about by the acceleration of rotation—caused by condensation according to the areolar theory—which it is supposed would be enormously in excess of the actual revolution of the inner planets, and of the rotation of the sun. Here we must protest against retrograde motion of rotation in any of the members of the solar system being considered as militating against the theory, because Laplace states distinctly, while explaining his hypothesis, that the rotation of the earth might just as well have been retrograde as direct: a fact that some eminent astronomers have not noticed, simply because they have not paid proper attention to what they were reading. We shall have to return to this statement again, and to present the proof of its being true.

An idea of how far the hypothesis had fallen into disrepute may be formed from the following extract, from "Nature" of August 4, 1887, of a Review of a "New Cosmogony," by A. M. Clerke, in which it is said: "But now the reiterated blows of objectors may fairly be said to have shattered the symmetrical mould in which Laplace cast his ideas. What remains of it is summed up in the statement that the solar system did originate somehow, by the condensation of a primitive nebula. The rest is irrecoverably gone, and the field is open for ingenious theorising. It has not been wanting. . . . The newer cosmogonists are divided into two schools by the more or less radical tendencies of the reforms they propose. Some seek wholly to abolish, others merely to renovate the Kant Laplace scheme. The first class is best represented by M. Faye, the second by Mr. Wolfe and Dr. Braun"—the author of the "New Cosmogony."

We cannot pass this quotation without remarking "How glibly some people can write!" More we do not want to say about it, except that it gave us the notion to examine closely some of the new cosmogonies, *which have not been wanting*, to see whether they are better than Laplace's.

We have not had the opportunity of knowing what are Mr. Wolfe's amendments, but the Review, just cited, gives us a pretty good notion of those of Dr. Braun, and we have been able to study carefully M. Faye's "Origine du Monde," in which he considers the solar system to have been evolved from cosmic matter partially endowed with motion in the form of eddies, whirlwinds, vortices, or *tourbillons*, which last may comprehend all of them, and even more. We have also studied, with some surprise, in "Climate and Cosmology" Dr. Croll's Impact, or Collision, Theory, and will confine our examination to the three of which we know something, beginning with Dr. Croll's, which we believe to be the oldest of the three.

We understand that Dr. Croll accepts the nebular hypothesis in all its main features, including the intense heat in which the original nebula is supposed to have existed from the beginning; and has only invented the collision theory in order to increase its quantity, to suit the demands of geologists for unlimited time, by showing how an unlimited supply of both heat and time may be obtained. But he has incurred an oversight in not taking into consideration the kind of matter in which that unlimited supply of heat was to be stored up—whether it would hold it. He wrote in times when something was really known about heat, and we cannot suppose him to have believed that heat could exist independent of matter, or that a gas or vapour could be heated to a high temperature except under corresponding pressure; but he has evidently overlooked this point, his thoughts recurring to old notions; and he has fallen, probably for the same reason, into other oversights equally as grave.

When showing how a supply of fifty millions of years of sun-heat could be produced from the collision of two half-suns colliding with velocities of 476 miles per second, Dr. Croll says in his "Discussions on Climate and Cosmology," of 1885, at page 301: "The whole mass would be converted into an incandescent gas" (the handmaid of the period), "with a temperature of which we can have no adequate conception. If we assume the specific heat of the gaseous mass to be equal to that of air (viz. 0·2374), the mass would have a temperature of about 300,000,000° C., or more than 140,000 times that of the voltaic arc."

Now, let us suppose the whole mass of the whole solar system to be converted into a gas, or vapour, at the pressure of our atmosphere, and temperature of 0° C., its volume would be equal to that of a sphere of not quite 9,000,000 miles in diameter. Suppose, then, this volume to be heated to 300,000,000° C. in a close vessel, as would necessarily have to be the case, the pressure corresponding to that temperature would be 1,094,480 atmospheres, according to the theory on which the absolute zero of temperature is founded. Without stopping to consider whether air or any gas could be heated to the temperature mentioned; or the strength of the vessel 9,000,000 miles in diameter required to retain it at the equivalent pressure; if we increase the diameter of the containing sphere to a little more than that of the orbit of Neptune, or, say 6,000,000,000 miles, and allow the air or gas or vapour to expand into it; then, as the volume of the new sphere will be greater than the former one in the proportion of 9,000,000 cubed to 6,000,000,000 cubed, or as 1 is to 296,296,296, the pressure of the gas will be reduced to 296,296,296 divided by 1,094,980, that is just over the 270th part of an atmosphere; which, in its turn would correspond to a temperature of a very little more than −273°, or what is considered to be* 273° C. above absolute zero of temperature; or, at all events, to the temperature of space, whatever that may be.

Dr. Croll goes on to say at page 302: "It may be objected that enormous as would be such a temperature, it would nevertheless be insufficient to expand the mass against gravity so as to occupy the entire space included within the orbit of Neptune. To this objection it might be replied, that if the temperature in question were not sufficient to produce the required expansion, it might readily have been so if the two bodies before encounter be assumed to possess a higher velocity, which of course might have been the case. But without making any such assumption, the necessary expansion of the mass can be accounted for on very simple principles. It follows in fact from the theory, that the expansion of the gaseous mass must have been far greater than could have

* This temperature is altogether erroneous, as we shall show in due time; at present our proof would not be accepted without a demonstration, for which we have not sufficient data.

resulted simply from the temperature produced by the concussion. This will be obvious by considering what must take place immediately after the encounter of the two bodies, and before the mass has had sufficient time to pass completely into the gaseous condition. The two bodies coming into collision with such enormous velocities would not rebound like two elastic balls, neither would they instantly be converted into vapour by the encounter. The first effect of the blow would be to shiver them into fragments, small indeed as compared with the size of the bodies themselves, but still into what might be called in ordinary language immense blocks. Before the motion of the two bodies could be stopped, they would undoubtedly interpenetrate each other; and this of course would break them up into fragments. But this would only be the work of a few minutes. Here then we should have all the energy of the lost motion existing in the blocks as heat (molecular motion), while they were still in the solid state; for as yet they would not have had time to assume the gaseous condition. It is obvious, however, that the greater part of the heat would exist on the surface of the blocks (the place receiving the greatest concussion), and would continue there while the blocks retained their solid condition. It is difficult in imagination to realize what the temperature of the surfaces would be at this moment. For supposing the heat were uniformly distributed through the entire mass, each pound, as we have already seen, would possess 100,000,000,000 foot-pounds of heat. But, as the greater part of the heat would at this instant be concentrated on the outer layers of the blocks, these layers would be at once transformed into the gaseous condition, thus enveloping the blocks and filling up the interstices. The temperature of the incandescent gas, owing to this enormous concentration of heat, would be excessive, and its expansive force inconceivably great. As a consequence the blocks would be separated from each other, and driven in all directions with a velocity far more than sufficient to carry them to an infinite distance against the force of gravity were no opposing obstacle in the way. The blocks, by their mutual impact, would be shivered into small fragments, each of which would consequently become enveloped in incandescent gas. These smaller fragments would in a similar manner break up into smaller pieces, and so on until the whole came to assume the gaseous state. The general effect of the explosion would be to disperse the blocks in all directions, radiating from the centre of the mass. Those towards the circumference of the mass, meeting with little or no obstruction to their outward progress, would pass outwards into space to indefinite distances, leaving in this manner a free path for the layers of blocks behind them to follow in their track. Thus eventually a space, perhaps twice or even thrice that included within the orbit of Neptune, might be filled with fragments by the time the whole had assumed the gaseous condition.

"It would be the suddenness and almost instantaneity with which the mass would receive the entire store of energy before it had time even to assume the molten, far less the gaseous condition, which would lead to such fearful explosions and dispersion of the materials. If the heat had been gradually applied, no explosions, and consequently no dispersion of the materials would have taken place. There would first have been a gradual melting; and then the mass would pass by slow degrees in vapour, after which the vapour would rise in temperature as the heat continued, until it became possessed of the entire amount. But the space thus occupied by the gaseous mass would necessarily be very much smaller than in the case we have been considering, where the shattered materials were first dispersed in space before the gaseous condition could be assumed."

We have made this very long quotation; first, because we have not been able to condense it without running the risk of not placing sufficiently clearly the whole of the argumentations employed in it; secondly, because the purport of the whole explanation set forth is evidently to demonstrate that, by means of the explosions of gases produced by the collision, the matter of the whole mass would be more extensively distributed into space—bearing heat along with it—than were it gradually melted and converted into vapour; and thirdly, because every argument advanced in favour of the theory of explosions, if carefully looked into, brings along with it its testimony that it has not been studied thoroughly out to the end. Thus the quotation in a great measure saves us that labour.

Dr. Croll seems sometimes to demand more from the laws of nature than they can give. He says, at p. 42 of the work cited, that the expansion of the gaseous mass, produced by the collision of the two bodies, must have been far greater than could have resulted simply from the temperature produced by the concussion; and goes on to show how it—the expansion—might be caused by explosions of gases blowing out blocks of matter in all directions to indefinite distances. But he forgets that these explosions of gases would consume a great part of the heat they contained, that is, turn it into motion of the blocks, and so diminish the quantity produced by the collision, just in proportion to the velocities given to the masses of all the blocks blown out; so that what was gained in expansion would be lost in heat, and the object aimed at—of producing heat for the expenditure of the sun—so far lost. Also, that, were the thing feasible, the blocks could not carry with them any of the heat of the exploded gases that might not be used up, and that the heat contained in them derived from the concussion would have time in their flight—about two hours at 476 miles per second—to melt the matter composing them and turn it into vapour, long before even the orbit of Neptune was reached. The heat produced by the explosion of powder in a cannon gives the projectile all the impulse it can, and disappears; it is converted into motion. It does not cluster round the projectile, nor follow it up in its flight, nor push it through an armour plate when it pierces one. We cannot admit—for this reason—the possibility of a block of matter flying off into space, with a mass of heat clustering round it, like bees when swarming round a branch of a tree. Thermodynamics does not teach us anything about a mass of heat sticking to the surface of a block of matter of any kind.

If the heat were, at a given moment—that is, when motion was stopped—brought into existence uniformly throughout the entire mass, which, according to the law of conversion of motion into heat and *vice versâ*, would most assuredly be the case, and each pound of the mass possessed 100,000,000,000 foot-pounds of heat, it could not be heaped up on the outer layers of the blocks—it matters not whether this means the layers of the outside of the whole mass, or at the outsides of the blocks—for the energy of lost motion, converted into heat, must have existed at the centres of the blocks or masses just in as great force as it did at the surfaces when motion was stopped. If each pound of matter carried along with it 100,000,000,000 foot-pounds of heat, that given out by one pound at the centre of a block would be as great as that given out by one pound at its surface; and the pounds at the surface could not acquire any greater heat from a neighbouring pound, because its neighbour could have no greater quantity to give it. Pounds of matter would be melted and vaporized, or converted into gas, just as readily at the centre of the mass or block as at its surface; and storing up of heat in the interstices of the blocks is rather a strange notion, because we are not at liberty to stow away heat in a vacuum. Besides, it is impossible to conceive how anything in the shape of a block could exist in any part of the whole mass, long enough for it to be blown out into space as a block. But supposing that a block could exist, it would most notoriously be in a state of *unstable equilibrium*; and were it then to receive from an explosion of gas, an impulse sufficient to drive it off to the verge of the sun's power of attraction—or rather to a distance equal to what that is—which would imply a velocity of not less than 360 miles per second, the shock would be quite sufficient to blow it into its constituent atoms. Moreover, as already stated, the heat of the explosion of the gas required to give the impulse would be immediately converted into motion, and disappear; so that out of the heat produced by the stoppage of a motion of 476 miles per second, that required to produce a motion of 360 miles per second, in each one of the blocks blown out to the distance above mentioned, would be entirely lost to the stock of heat schemed for so boldly. Of course, the less the distance from the centre the blocks were blown the less would be the loss, but the fact remains that there would be a loss instead of a gain of heat, in dispersing the matter of two half suns into space by explosions of gas. In fine, a given amount of heat will raise the temperature of a given amount of matter to an easily calculable degree, and no more; and if part of that is expended in expanding the volume of the matter, the whole stock of heat will be diminished by exactly the quantity required to produce the expansions. So that we come back to what we have said at page 54, viz., that when the matter and the heat of the collision of the two half suns were dispersed, under the most favourable circumstances, into a sphere of 6,000,000,000 miles in diameter, the mean density of the matter would be equal to about $\frac{1}{270}$th part of an atmosphere, and its temperature—what is called—273° C. of absolute temperature, always considering the quantity of the heat to have been 300,000,000° C.

Dr. Croll says that if a velocity of 476 miles per second were not sufficient to produce the quantity of heat required, any other necessary velocity might be supposed, but when we consider that his supply of 300,000,000° C. would have to be increased to 82,000,000,000° C., in order to add 1° C. of heat to the matter dispersed through a sphere of 6,000,000,000 miles in diameter, it seems unnecessary to pursue the subject any farther.

We may now take a look at Dr. Braun's Impact Cosmogony, of which we know nothing beyond what is set forth in the Review in "Nature" already alluded to, but that is enough for our purpose. We understand that he extends his operations to the whole universe, which he conceives to have been formed out of almost unlimited, and almost imponderable, nebulous matter, not homogeneous, but with local irregularities in it, which "would lead to the breaking up of the nebula into a vast number of separate fragments." Out of one of these fragments he supposes the solar system to have been formed. This fragment would contain local irregularities also, which through condensation would lead to the formation of separate bodies, and these bodies are supposed to have been driven into their present forms, and gyrating movements of all kinds, by centric and eccentric collisions among themselves, caused by their mutual attractions. Of course anything can be supposed, but in a construction of this kind the idea is forced upon us of the necessity of the active superintendence of the Creator, to create in the proper places and bring in the matter at the exact moment required, and to see that the collisions were directed with the proper degree of energy and eccentricity, to construct the kind of machine that was proposed. To this idea we have no objections whatever, but we would like to see the necessity for it acknowledged. Perhaps Dr. Braun does acknowledge it, but the cosmogony is given to us, it would seem, to show what most probably was the original scheme of construction, and implying that no continual supervision and direction were required during the process. If Dr. Braun could show us some method of attraction, and suspension and variation of attraction, by which some of the separate bodies could be drawn towards each other so as to form a central mass, nebula, or sun, and to give it, by their impacts of collision, a rotary motion; and how others of the separate bodies could be formed and held in appropriate places, so as to be set in motion at the right moment; and how they were to be so set in motion without the direct action of the constructor, to revolve as planets around the central mass, we might be able to recognise that a mechanism such as that of the solar system might be brought into existence; but when we are left to discover all these requisites, and their *modus operandi*, we find that we might be as well employed in designing a cosmogony of our own.

Dr. Braun indulges in somewhat startling numbers in temperature and pressure. He considers that the temperature of the sun, at the surface, may be from 40,000° to 100,000° C., and that it may reach to from ten to thirty million degrees at the centre. In this he may be right for anything we know to the contrary. When riding over a sandy desert, under an unclouded vertical sun, we could easily have believed anything of the central heat of such a fire, especially when we considered that it was at a distance of ninety-three millions of miles from us. But when he tells us that in the depths of the sun's interior the pressure reaches a maximum of two thousand millions of atmospheres, we "pull in resolution and begin to doubt." Air at that pressure would have a density 2,585,984 times that of water, or 456,887 times the mean density of the earth, and we should have a species of matter to ponder over, of which no physicist has ever as yet dreamt.

We have been able to study M. Faye's cosmogony in his work on "L'Origine du Monde," second edition of 1885, and can give a better account of it than of Dr. Braun's.

(1) He repudiates almost all existence of heat in the cosmic matter he is about to deal with, recognising that its temperature must have been very near the point of absolute zero, and also that its tenuity must have been almost inconceivable ; so tenuous that a cubic miriamètre of it would not contain more perhaps than 5·217 grammes in weight. And very properly, we think, he looks upon the solar systems as having, at one time, formed a part of the whole universe, all of which was brought into existence, created, more or less, about the same time. In this universe, he considers that the stars have been formed, as well as the sun, by the progressive concentration of primitive materials disseminated in space, which conception gives rise to a totally new notion of the most positive character : viz. that each star owes to its mode of formation a provision of heat essentially limited ; that it is not permissible, as Laplace thought he could do, to endow a sun with an indefinite amount of heat ; and that what it has expended and what it still possesses, depend upon its volume and actual mass. And also that the primitive materials of the solar system were, at the beginning, part of a universal chaos from which they were afterwards separated, in virtue of movements previously impressed on the whole of the matter ; and sums up his first ideas in the following manner or theorem :

"*At the beginning the universe consisted of a general chaos,* *of extreme tenuity, formed of all the elements of Chemistry more or less mixed and confounded together. These materials under the force of their mutual attractions were, from the beginning, endowed with diverse movements which brought about their separation into masses or clouds. These still retained their movements of rapid translation, and very gentle interior gyrations. These myriads of chaotic fragments have given birth, by means of progressive condensations, to the diverse worlds of the universe.*"

(2) So much for the formation of the universe, including, of course, the solar system, for which he acknowledges the necessity for the intervention of a creating power, because it is impossible to account for it simply by the laws of nature; and adds: It is unnecessary to say that the universe is an indefinite series of transformations, that what we see results logically from a previous condition, and thus necessary in the past as in the future; we cannot see how a previous condition could tend towards the immense diffusion of matter, to the chaos out of which the actual condition has arisen; and that it is, therefore, necessary to begin with a hypothesis, and postulate of God, as Descartes did, the disseminated matter and the forces which govern it.

(3) From dealing with the universe, M. Faye comes to the formation of an isolated star, and begins with an entirely ideal case, that of a spherical homogeneous mass, without interior movement of any kind, and concludes that the molecules would fall in straight lines towards the centre; that the mass would condense regularly without losing its homogeneity, and would end in producing an incandescent sphere perfectly immovable; and that that would be a star, but a star without satellites, without rotation, without proper movement. This not being what was wanted, he goes on to show how, previous to its separation and complete isolation from the universal chaos, such a mass would possess, and carry with it when separated, a considerable velocity of rotation, and would still retain the internal movements it had acquired from the attraction of the other masses with which it had been previously in contact; and how the molecules, drawn towards the

centre in obedience to gravitation, would not fall in straight lines but in concentric ellipses.

(4) From this state of affairs, two very different results might arise. One, that the molecules might resolve themselves into a multitude of small masses without the centre acquiring a preponderating increase. The other, that the central condensation might greatly exceed the others, and there would be formed a central star accompanied by a crowd of small dark bodies. M. Faye accepts the second result, in which case the ellipses described by the small bodies, now become satellites, would, as the central mass increased in preponderance, have one of their centres at the centre of the preponderating mass, and their times of revolution would vary from one to another in conformity to the third law of Kepler.

(5) For the formation of the solar system M. Faye finds that it is of little importance whether the movements of bodies around the sun be very eccentric or almost circular ; the first cause is always the same. They arise from the eddies, *tourbillonnements*, they have brought with them from their rectilinear movements in the primitive chaos. But the circle is such a particular case of the ellipse, that we ought not to expect to see it realized in any system. It is therefore necessary that, among the initial conditions of the chaotic mass, one should be found which would prevent the gyrations, eddies, from degenerating into elliptical movements, and which has at first made right, and afterwards firmly preserved, the form, more or less circular, in all its changes.

(6) For the formation of circular rings he gives us the following conceptions : In order that a star should have companions, great or small, circulating round the centre of gravity of the system, it is necessary that the partial chaos from whence it proceeded should have possessed, from the beginning, a gentle eddying movement affecting a part of its materials. Besides, if the partial chaos has been really round and homogeneous, we shall see that these gyrations must have taken up, and to some extent preserved, the circular form. He then requests the reader not to lose sight of the feeble density of the medium, in which a succession of mechanical changes are

to be brought about; and not to conclude that that density was such that a cubic miriamètre of the space occupied by it might not contain 3250 grammes of matter, as he stated in the preceding chapter (we think he said 5217 grammes), but that it might contain only 3 grammes or even less. And adds that in such a medium, the small agglomerations of matter which would be formed all through it, would move as if they were in an absolute vacuum, and any changes in them would be produced extremely slowly.

(7) Then he goes on to say that the gyrating movements belonging to the chaotic mass, would have very little difficulty in transforming a part of a motion of that kind into a veritable rotation, if this last were compatible with the law of the internal gravitation; that it is the nature of that kind of masses to only permit, to the bodies moving in them, revolutions, elliptic or circular, concentric and of the same duration; that therefore notable portions of the gyrating matter could take the form and movements of a flat ring, turning around the centre with the same angular velocity, exactly as if this nebulous ring were a solid body; that all the particles which have the proper velocity in the plane of the gyrations, will arrange themselves under the influence of gravitation in a flat ring with a veritable rotation around the centre; that any other parts having velocities too great or too small, will move in the same plane, describing ellipses concentric to the ring; that if the ellipses are very elongated the materials composing them will approach the centre, where they will produce a progressive condensation, communicating to the central globe formed there a rotation in the same plane with the primitive gyrations; and finishes off the whole scheme by specifying the first results to be: (1) The formation of concentric rings turning in one piece, in the manner of a solid body, around a centre almost empty (*d'abord vide*); and (2) A rotation in the same direction, communicated to the condensation which would be produced, little by little, by means of matter coming in, partly, from regions affected by the internal eddyings (*tourbillonnements*).

(8) It is unnecessary to go any farther, and take note of

his method of the formation of planets and satellites from rings, as it is much the same as what we have seen described by others who have written on the same subject ; only interpreted by him in a way to suit his own purposes, and in which interpretation he does not do full justice to Laplace, through not having paid sufficient attention to his explanation of how planets could be formed out of rings. Except in so far as to note that all along he has considered that rings were formed, and even those nearest to the centre condensed into globes, long before the central condensation had attained any magnitude of importance, or assumed any distinctive shape, and that afterwards all the disposable matter of the rings and also all the exterior matter that had not formed part of what was separated from the original universal chaos, had fallen in towards the small central mass, and so completed the formation of the sun last of all.

We shall now proceed to make a few remarks with respect to this condensation of M. Faye's cosmogony, which we think we have made without adding to or omitting anything of importance that we have met with in his work, for which purpose we have numbered the paragraphs containing it, in the last six pages, in order to do away with the necessity of repeating the parts to which we refer.

No. 1. All those who believe that "the solar system did originate somehow, by the condensation of a primitive nebula," agree with M. Faye in considering that the density of the nebulous matter must have been extremely low, and some of them seem almost to vie with each other in showing how great must have been the degree of its tenuity ; but M. Faye is one of the few who, paying due respect to the law of the interdependence of temperature and pressure in a gas or vapour, maintain that it must have been almost devoid of temperature, and we have to acknowledge that he is in the right. Then we believe that his assumption, that the whole universe of stars, including the sun, was created, humanly speaking, about the same time, is shared by the great majority of those who have thought at all seriously on the subject. Also, we agree with him firmly in his statement that each star—and we add planet, satellite, etc.—was originally supplied with an extremely limited quantity of heat, and that what it has expended and what it still retains has been derived entirely from the condensation of the original cosmic matter out of which it was made.

With regard to his theorem: we cannot follow him in his statement that the diverse movements caused by the mutual attractions of parts of the original universal mass of cosmic matter, have brought about its separation into myriads of fragments; nor how these fragments could carry with them a rapid movement of translation, unless the whole universal mass was endowed with a rapid movement of translation through space, in which case we think that such a motion would have had no greater particular effect in producing new forms of motion in the fragments, than if the whole had been created in a state of rest. Stray movements of translation might give rise to collisions among the multitude of fragments, and perhaps that was one of the modes of formation into suns through which they had to pass; but we cannot follow it out. Neither can we see clearly how translation could be effected of one mass into the space occupied by another mass—unless empty spaces were reserved for that purpose from the beginning. Without that, translation could not exist: it would be collision.

No. 2. We have nothing to object to what is said in this paragraph; except that a rotating sphere might have been postulated at once, in imitation of Laplace, instead of trying like Descartes to join fragments together, endowed with movements so adjusted that, among the whole of them, they would produce in the whole mass, when united, the kind of movement that was wanted.

No. 3. To the ideal case of the formation of an isolated sun from a homogeneous mass without interior movement of any kind, we cannot agree in any way. The molecules of matter would not, could not, fall in towards the centre in straight lines. Their mutual collisions would drive them generally in curved lines in all directions as they fell in, which would create new internal movements; and these movements would prevent the possibility of the formation of an immovable incandescent sphere such as is described. There could be no immobility in the interior of a sun, as long as its temperature was sufficient to keep the surface incandescent. But we cannot give our reasons here for this assertion—to most people they will, we think, occur at once—because we have a long road to travel before we can do so.

When M. Faye abandons the isolated case, he leaves us without giving us any help, to conceive for ourselves how the mass would possess and carry with it a considerable velocity of rotation, and still retain the internal movements it had acquired from the attraction of the other masses—of the universal chaos—with which it had been in contact; and also how the molecules drawn towards the centre would not fall in straight lines but in concentric ellipses. And this last we have to do without his giving us any reason why the molecules should fall in towards the centre at all; or rather in spite of the fact that one of his principal ideas would lead us to expect exactly the contrary, as we shall see presently.

No. 4. Here he places before us again, two cases in one of which the molecules might resolve themselves into a multitude of small masses, without the centre acquiring any preponderating increase; and the other where the central condensation might greatly exceed the others, and there would be formed a central star accompanied by a crowd of small dark bodies, now become satellites, describing ellipses around the central preponderating mass. This second case he seems, for the time being, to accept as the most probable; but it is strangely at variance with what he sets forth afterwards. He does not give us the least hint as to why or how the satellites acquired their various times of revolution, but only assumes that they did so; and we are very sure that it was not the third law of Kepler that was the agent in the case, however much it might suit his purpose.

No. 5. Although this part of his exposition is dedicated to the formation of the solar system, all that M. Faye says is that it is of little importance whether the movements of bodies around the sun be very eccentric or almost circular;

and that among the initial conditions of the chaotic mass, all that we require is that one should be found which would prevent the gyrations from degenerating into elliptic movements, and which had first put right and afterwards firmly preserved the form, more or less circular, in all its changes. But he does not make any attempt to show what that one condition is, and allows us to find it out for ourselves.

No. 6. What M. Faye says about the formation of circular rings is more or less a repetition of what he has adduced, to explain all the other movements which he has derived from the universal chaos ; and which he seems to think sufficient to account for such movements being nearly circular. For our part we do not think they are sufficient, and he does not show us how they influence each other to bring about the final movements he wants to present to us.

We duly take note of the tenuity of the cosmic matter on which he operates, which at 3 grammes in weight to 1 cubic miriamètre would correspond to one grain in weight to 771,947,719,300 cubic feet of space, or 1 grain to a cube of 9173 feet—more than 3000 yards—to the side. We do this in order to remind him of what he says at page 151 of his work, when dealing with the rotation of the Kant-Laplace nebula—namely, that it is impossible to comprehend how an immense chaos, of almost inconceivable tenuity, could possess such a rotation from the beginning, and that for want of that inadmissible supposition nothing remains to fall back upon but the *mouvements tourbillonnaires* of Descartes. Thus he wants us to believe that his *tourbillons* could move in straight or curved lines, have motions of translation, could attract, restrain, and drive each other into all sorts of movements with the tenuity he has indicated ; but that Laplace's nebula, with a density of 1 grain to a cube of 90 feet—or at most 150 feet—to the side, could not be conceived to have the single movement of rotation. And lastly, we repeat that if the centre of the chaos was almost empty, we do not see what induced the cosmic matter to fall into it in elliptic orbits.

Nos. 7 and 8. In these paragraphs, the main features are repetitions of the simple assertions made in all the others, that certain movements possessed by matter in one state would produce other movements in another state, without attempting to show how they all came to so far coincide with each other and form one harmonious whole, with movements in almost one single direction. It is clear that one side of the separated chaos might have acquired motion in one direction from the universal chaos with which it had been in contact, and that the opposite side might have acquired motion in exactly the opposite direction from the original chaos with which it had been in contact; and we are left to find out how these came to agree with each other in the end. And, going back to the beginning, we are left to find out where the mass, out of which he constructs his solar system, was stowed away after it was separated from the original universal chaos. We can conceive of its being separated by condensation, in obedience to the law of attraction, from the surrounding chaos, in which case it might fall towards a centre, or that some parts of it might come to revolve round each other, and that finally the whole of these parts might come to rotate about a common centre; but that is evidently very different from the mode of formation of the solar system which M. Faye has advocated. It comes to be by far too like the nebula which Laplace supposed to be endowed with rotary motion from the beginning, probably because he did not see, or did not take the trouble to see, how such a motion could be produced. In any case, Laplace did not consider that the primary motion of rotation was the most important part of his hypothesis; neither was it, as it seems to have been in the case we have been considering. And he did not go much further than M. Faye in postulating primary motion, only he did it in a more effectual and business-like manner. He drew on the bank at once for all the funds he required, instead of having to draw afresh every time he found himself in difficulties, as has been the lot of his critic and successor.

Finally, M. Faye tries to show that after all his rings, flat or otherwise, converted or not converted into globes, had been formed according to his ideas, the greater mass by far of the chaos had fallen into the centre, and had formed the sun there last of all. Now, if the preponderating mass of the chaos had been outside of the field of his operations, up to the period when all his planets, satellites, etc. were formed, or at least laid out, it is more natural to suppose that the matter inside of his structure, if there was any, would be drawn outwards by the attraction of the greatly preponderating mass outside, than that any portion of it should have fallen in, in elongated ellipses, towards the insignificant mass that he supposes to have been inside his structure. This, of course, would be nearly exactly the reverse of the mode of formation he was trying to demonstrate, and clearly shows that he was working on unsound principles from the beginning to the end of his cosmogony. It had never occurred to him that matter could be attracted outwards as well as inwards, most probably because it would seem to him ridiculous to imagine that anything in the universe could *gravitate* upwards.

There are other theories of the formation of the solar system from meteorites and meteors, giving us the idea of its being made out of manufactured articles instead of originally created raw material, which does not in any way simplify the process. In some of them, the inrush of meteor swarms is invoked as the cause of gyratory motion, which places them in much the same category as impact theories. We know that broadcloth is made out of woollen yarn, but we also know how the yarn is made out of wool, and how it is woven into the cloth, whereas we are not told by what process, or even out of what the meteors and meteorites are made, although some of them are said to have thumb-marks upon them.

All these theories and cosmogonies may be very appropriately classified as variations of the nebula hypothesis, and like variations in another science, may be very brilliant, scientific, imaginative, grand, but after all the flights of fancy exhibited by them are set before us, we feel in a measure relieved when a return is made to the original air. They all assume original motion, varied, accidental, opportune, more dependent upon the will of the cosmogonist than on the laws of nature, which tend to confound rather than enlighten any one who tries to understand and bring them, mentally, into actual operation. Laplace assumed rotary motion for the whole of his nebula, and was thus able to account at once for the relation which exists among the planets in respect of distance from, and period of revolution around the sun—arising from the original rotation of the whole mass in one piece—a result which, in any impact theory, has to be accounted for separately, and, in plain truth, empirically in each case, and at each step.

Seeing, then, that we have not been able to find any cosmogony, or speculation, that gives us a more plausible idea of how the solar system has been formed, we shall try whether from the original nebula as imagined by Laplace, it is possible to separate the various members, and form the system in the manner described in his celebrated hypothesis. In other words, we shall endeavour to analyse the hypothesis.

CHAPTER IV.

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Preliminaries to Analysis of the Nebular Hypothesis.

It may be thought that there is little benefit to be derived from analysing an hypothesis which has been declared, by very eminent authorities in the matter treated of, to be erroneous in some points of very serious importance ; but hypotheses are somewhat of the nature of inventions, and we know that it has often happened that many parties, aiming at the same invention, have altogether failed, while some other person using almost exactly the same means as his predecessors, has been entirely successful in his pursuit. How many times has it been pointed out to us, that if such a person had only gone one step further in the process he was following, or had only studied more deeply the matter he had in hand, he would have anticipated by many years one of the greatest discoveries of the age ! In some cases the failure to take that one step was occasioned through want of knowledge acquired long years afterwards ; whereas we think that in the case we have in hand, it can be shown that the want of knowledge acquired many years after he had formulated his hypothesis, or if otherwise, the want of faith in what he knew, enabled Laplace to construct an edifice which otherwise he could hardly have convinced himself could be built up in a practical form. We think also that if he had made the proper use of the knowledge he must have had of the law of attraction, he would have seen that no nebula could ever have existed such as the one he assumed, extending far beyond the orbit of the remotest planet. Furthermore, we think it can be shown that if he had thoroughly considered what must have been the interior construction of his nebula, he would have found one that would have suited his hypothesis in the main point, viz. condensation at the surface, at least equally as well as endowing it with excessive heat. But to be able to show these things our first step must be to analyse the hypothesis, to examine into it as minutely and deeply as lies in our power.

For this purpose it will be necessary to define what the hypothesis is. Many definitions have been given, more or less clear, and it would be only a waste of time to try to set forth Laplace's own exposition of it, with all its details, which he had no doubt studied very carefully. But in those definitions that have come under our observation, several of the conditions he has specified are wanting, or not made sufficiently prominent ; so instead of adopting any one of them we will make a sort of condensation of the whole, adding the conditions that have been left out ; because the want of them, has been the cause of mistaken conceptions of the evolution of the system having been formed by very eminent astronomers. Our definition will therefore be as follows :—

1. It is supposed that before the solar system was formed the portion of space in which its planets and other bodies now perform their revolutions and other movements, was occupied by an immense nebula of cosmic matter in its most simple condition—of molecules or atoms—somewhat of a spherical form, extending far beyond its present utmost limits, and that it was endowed with excessive heat and a slow rotary motion round its centre ; which means that while it made one revolution at the circumference it also made one at the centre. The excessive heat, by counteracting in a certain measure the

force of gravitation, kept the molecules of matter apart from each other ; but as the heat was gradually radiated into space, gravitation became more effective, and then began to condense and contract more rapidly, by which process its rotary motion was, in accordance with the areolar law, gradually increased at the surface, *in the atmosphere of the sun*, where the cooling took place, and condensation was most active ; and the increase of rotation was propagated from there towards the centre.

(2) As the contraction and rotation increased a time or times arrived, when the centrifugal force produced by the rotation came to balance the force of gravitation, and a series of zones or rings were separated from the nebula, each one of them continuing to rotate—revolve now—around the central mass, with the same velocities they had at the times of their separation ; until at last the nebula became so contracted that it could not abandon any more rings, and what of it remained condensed and contracted into a central mass which ultimately assumed the form of the actual sun.

(3) In the meantime, or following afterwards, each one of the rings which were abandoned by the nebula, acquired, through the friction of its molecules with each other, an equal movement of revolution throughout its entire mass, so that the real velocities of the molecules furthest removed from the centre of the nebula were greater than those of the molecules nearest to its centre, and the ring revolved as if it were in one solid piece. Arrived at this stage the rings broke up and formed themselves into smaller nebulæ, each of which condensed into a globe or planet, and continued to revolve around the central mass in the same time as its mass had done when in the form of a ring. And some of these sub-nebulæ, imitating the example of their common parent more perfectly than others, abandoned in space in their turn smaller rings which in the same manner condensed, broke up, and formed themselves into smaller globes or satellites ; all, as far as we know, except the rings of Saturn, which have not as yet been converted into satellites.

(4) All of these bodies, planets, satellites, and rings were

TABLE I.

ELEMENTS AND OTHER DATA OF THE SOLAR SYSTEM EMPLOYED IN THIS ANALYSIS.

PART I.—SUN AND PLANETS.

Name.	Mean Distance from Sun in Miles.	Equatorial Diameter in Miles.	Volume in Cubic Miles.	Density. (Water = 1.)	Volume at Density of Water in Cubic Miles.	Time of Revolution round Sun in Days.
Sun	..	867,000	341,237,637,800,000,000	1·413	482,169,000,000,000,000	..
Mercury	35,987,000	2,957	13,537,968,847	6·850	92,735,000,000	87·9692
Venus	67,245,000	7,660	235,334,728,260	4·810	1,131,960,000,000	224·7007
Earth	92,965,000	7,918	259,923,832,335	5·660	1,471,169,000,000	365·2563
Mars	141,650,000	4,185	38,378,333,333	4·188	160,728,460,000	686·9796
Supposed planet	260,300,000	..	.. ..	..	367,792,000,000	1,714·1876
Jupiter	483,678,000	87,680	352,940,162,601,626	1·358	479,292,741,000,000	4,332·2548
Saturn	886,779,000	73,713	209,716,183,575,000	0·736	154,351,000,000,000	10,759·2198
Uranus	1,783,383,000	33,563	19,796,209,090,910	1·302	25,874,664,000,000	30,688·5076
Neptune	2,794,000,000	36,620	25,713,106,508,876	1·132	29,107,237,000,000	60,186·6385

TABLE I.—*continued.*

PART II.—SATELLITES OF PLANETS.

Names.	Mean Distance from Primary in Miles.	Equatorial Diameter in Miles.	Volume in Cubic Miles.	Density. (Water = 1.)	Volume at Density of Water in Cubic Miles.	Total Volume at Density of Water in Cubic Miles.
			Of the Earth.			
Moon	238,833	2160	5,276,682,926	3·438	..	18,141,236,000
			Of Jupiter.			
Jo	267,380	2252	5,980,050,000	1·132	6,769,416,600	
Europa	425,160	2099	4,842,133,708	2·141	10,367,008,269	
Ganymede	678,390	3436	21,240,229,268	1·868	39,676,748,273	
Callisto	1,192,820	2929	13,157,027,273	1·472	19,367,144,146	76,180,317,288
			Of Saturn.			
Mimas	120,800	1000	523,600,000	..		
Enceladus	155,000	?	65,450,000	..		
Tethys	191,000	500	65,450,000	..		
Dione	246,000	500	65,450,000	..		
Rhea	343,000	1200	904,780,417	..		
Titan	796,000	3300	18,816,606,060	..		
Hyperon	1,007,000	?	3,053,634,965	..	Total Volume in Cubic Miles.	
Japetus	2,314,000	1800	3,053,634,965	0·7 36	26,548,606,407	19,539,774,315

Of Uranus.

Ariel	123,000	Total mass taken at $\frac{1}{15,000}$th of primary	1,724,977,600
Umbriel	171,000		
Titania	281,000		
Oberon	376,000		

Of Neptune.

——	220,000	Mass taken at $\frac{1}{40,000}$th of primary	727,680,925

PART III.—RINGS OF SATURN.

Rings.	Diameters of Rings in Miles.		Areas of Rings in Square Miles.	Thickness of Rings in Miles.	Volume of Rings in Cubic Miles.	Density. (Water = 1).	Volume at Density of Water in Cubic Miles.
Outer . .	Outer . .	172,240	5,252,035,427				
	Inner . .	151,590					
Middle . .	Outer . .	148,100	6,919,075,757				
	Inner . .	114,560					
Dark . .	Outer . .	110,060	3,040,689,488				
	Inner . .	90,993					
		Total . .	15,211,800,672	90	1,369,062,060,480	·0001425	195,000,000

supposed to revolve around their primaries, and to rotate on their axes, in the same direction viz., from right to left, in the opposite direction to the hands of a watch.

In addition to the above definition it is necessary to give some sort of description of the various parts of the machine or system which has to be made out of the nebula, with their positions, dimensions, and details. This we believe will be made plain enough, in the simplest manner, by Table No. I., taken and calculated from the elements of the solar system given in almost all astronomical works, from which we have selected what be believe to be the most modern data.

The construction of this table requires some explanation on account of its being made to show complete results from incomplete data. There has been no difficulty with the sun, the major planets, and the satellites of the earth and Jupiter, but for the minor planets, the satellites of the three outer planets, and the rings of Saturn, we have been obliged to exercise our judgment as best we could.

There being almost no data whatever of the dimensions and densities of the minor planets, to be found, we have been driven in order to assign some mass to them, to imagine the existence of one planet to represent the whole of them (in fact Olbers's planet before it exploded), which we have supposed to be placed at the mean distance of 260,300,000 miles from the centre of the sun; and we have given to it a mass equal to one-fourth of the mass of the earth, that being, in the opinion of some astronomers, the greatest mass which the whole of them put together could have. This assumption we shall explain more fully at a more suitable time.

In the case of Saturn the diameters of two of the satellites are wanting which we have assumed to be the same as those of the smallest of those nearest to them, and thus have been able to compute the volumes of the whole of them; but we have not been able to find any statement anywhere of their densities, and to get over this difficulty we have reasoned in the following manner.

The density of the moon is very little over two-thirds of that of the earth, while that of the satellites of Jupiter varies from a little more than the same to a little more than twice as much as the density of their primary. Why this difference? To account for it we appeal to the very general opinion of astronomers, that the four inner planets are in a more advanced stage of their development, or existence, than the four outer ones. In this way it is easy to conceive that the earth has arrived at the stage of being more dense than its satellite; while in the case of Jupiter, his satellites being of so very much less volume than their primary, have already arrived at a higher degree of development. Carrying this motion forward to Saturn, we have supposed that from his being considerably less dense than any other of the outer planets—quite possibly from having been formed out of material comparatively (perhaps not actually) less dense than the others—his satellites may not have condensed to a greater degree than his own mass, and we have, therefore assumed their density, that is the density of the volume of the whole of them, to be the same as that of their primary.

To determine some mass for the rings of Saturn, is a much more intricate matter than for his satellites, and presents to us some ideas—facts rather—which had never before crossed our imagination. The most natural way to look upon these rings is to suppose that they are destined to become satellites at some future time. All the modern cosmogonies that have come under our notice are founded upon the idea that rings are the seed, as it were, of planets and satellites, and if those of Saturn have been left, as it has been said, to show how the solar system has been evolved, it cannot be said that the supposition is not well founded. In this way we are led to speculate upon how many satellites are to be made out of the rings before us. Considering, then, that the nearest satellite is 120,800 miles from the centre of Saturn, leaving only 83,500 miles between his surface and that of Mimas, and also that the distances between satellites diminish rapidly as they come to be nearer to their primaries, there is not room to stow away a great number of satellites. On the other hand, seeing that there are at least three distinct rings, we cannot reasonably do less than conclude that three satellites are intended to be made out of them. But let the number be what it may, all that we have to do with them for our present purpose is to assign some mass to them. With this view, we have given, arbitrarily, to each one of the three we have supposed, a volume equal to that of one of the satellites of 500 miles in diameter, that is, about 65,000,000 cubic miles, and we have supposed their density to be the same as that of water, instead of that of the planet. Thus, in the table, we have assigned to the three a mass of 195,000,000 cubic miles at density of water, which would be more than sufficient to make four other satellites for the system of 500 miles in diameter each, and of the same density as the planet.

For the table referred to we have calculated the areas of the three rings to be 152,110,800,172 square miles, and we have assumed the thickness as 90 miles, that is about two-thirds of that estimated by Chambers in his hand-book of Astronomy, but almost the same as that given by Edmund Dubois ; nevertheless their total volume comes up to 1,369,062,060,480 cubic miles, which reduces their average density to 0·0001425 that of water, to make up the mass of 195,000,000 cubic miles at the density of water, which we have adopted for the three. This density corresponds to very nearly one-tenth of that of air, which, however strange it may appear to us, may be considered to be a very full allowance, seeing that we shall find, later on, that the planet itself was formed out of matter whose density could not have been more than one twenty-six millionth part of that of air. All the same, it is hardly matter that we could liken to brickbats. After being driven to this low estimate of density, which startled us, we referred to an article in "Nature" of Nov. 26, 1886, on Ten Years' Progress in Astronomy, where we find what follows :—"He (Newcomb) finds the mass of Titan to be about $\frac{1}{12.000}$ that of Saturn. It may be noted, too, that Hall's observations of the motions of Mimas and Enceladus indicate for the rings a mass less than $\frac{1}{10}$ that deduced by Bessel ; instead of being $\frac{1}{100}$ as large as the planet, they cannot be more than $\frac{1}{1000}$, and are probably less than $\frac{1}{10.000}$." (We make them $\frac{1}{791514}$). Thinking over the numbers herein given we cannot help being surprised by them. If Titan be $\frac{1}{12500}$ of the mass of Saturn, we cannot conceive how the mass of his rings can be so much greater than that of Titan. We cannot pretend to fit even one satellite of that size, mechanically, into a space of 83,500 miles wide, while Titan revels in an ample domain with a width of 332,000 miles. But we shall not pursue this part of our speculations any further. Astronomers may be able to demonstrate that the rings are of a totally different nature to those out of which the planets and their satellites are supposed to have been made, or that the nebular hypothesis or anything resembling it is no better than a foolish dream. All that we have pretended to do has been to give them their due place in the hypothesis we are attempting to analyze, and to look upon them in a practical and mechanical light, as an unfinished part of the solar system.

To determine masses for the satellites of the two outer planets, we have to be more empirical even than we have yet been. A little trouble will show that the whole mass of all the satellites and rings of Saturn put together is about $\frac{1}{7820}$th of the mass of the planet, and we shall avail ourselves of this proportion to assign masses for the satellites of the remaining planets, the numbers and names of which are the only data we have been able to find. Considering then, that Uranus has only four satellites and no rings, we think if we give them $\frac{1}{15,000}$th of the mass of their primary, it will be a very fair allowance ; and with the same empiricism we have adopted for the solitary satellite of Neptune $\frac{1}{40,000}$th of the mass of its primary.

However rude and crude these approximations may be, we have the satisfaction of thinking that the masses obtained by their means, can have no appreciable effect upon the operations into which they are to be introduced, whilst they enable us to deal with a complete system or machine. But for these we have another Table No. II. to present, a *résumé* of the foregoing one, for greater facility of reference.

TABLE II.—VOLUMES OF THE VARIOUS MEMBERS OF THE SOLAR SYSTEM AT THE DENSITY OF WATER.

Name.	Designation.	Volume in Cubic Miles at Density of Water.	Total Volume in Cubic Miles at Density of Water.
Sun . . .	..	..	482,169,000,000,000,000
Mercury .	Planet	..	92,735,000,000
Venus . .	,,	..	1,131,960,000,000
Earth . .	,,	1,471,169,000,000	
Moon . .	Satellite	18,141,236,000	1,489,310,236,000
Mars . .	Planet	..	160,728,460,000
—— . .	Asteroids	One fourth of Earth	367,792,000,000
Jupiter . .	Planet	479,292,741,000,000	
,, . .	4 Satellites	76,180,317,000	479,368,921,317,000
Saturn . .	Planet	154,351,000,000,000	
,, . .	8 Satellites	19,539,774,315	
,, . .	3 Rings	195,000,000	154,370,734,774,315
Uranus . .	Planet	25,874,664,000,000	
,, . .	4 Satellites	1,724,977,600	25,876,388,977,600
Neptune .	Planet	29,107,237,000,000	
,, .	1 Satellite	727,680,925	29,107,964,680,925
Total of Planets, Satellites and Rings . .			691,966,535,445,840

Dividing 482,169,000,000,000,000 by 691,966,535,445,840 makes the mass of the whole of the members to be $\frac{1}{696 \cdot 86}$th part of the mass of the sun, instead of $\frac{1}{700}$th as generally stated by astronomers.

CHAPTER V.

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86 Temperature of the nebula not far from absolute zero.
86 Erroneous ideas about glowing gases produced by collisions of their atoms, or particles of cosmic matter in the form of vapours.
87 Separation of ring for Neptune. It could not have been thrown off in one mass, but in a sheet of cosmic matter.
88 Thickness and dimensions of the ring.
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98 Residual mass. Condensation of Solar Nebula to various diameters, and relative temperatures and densities.
100 Unaccountable confusion in the mode of counting absolute temperature examined and explained. Negative 274 degrees of heat only equal 2 degrees of absolute temperature.
103 The Centigrade thermometric scale no better than any other, and cannot be made decimal.
104 The sun's account current with the Nebula drawn up and represented by Table III.

Analysis of the Nebular Hypothesis.

We may now proceed to take the original nebula to pieces, by separating from it all the members of the solar system, in performing which operation we shall suppose the divisions between the nebula and each successive ring to have taken place at a little more or less than the half distances between the orbits of two neighbouring planets, because we have no other data to guide us in determining the proper places. These divisions have manifestly been brought about in obedience to some law, as is proved in great measure by what is called

Bode's Law; although no one has as yet been able to explain the action of that law. It is no doubt certain that a division must have taken place much nearer to the outer than the inner planet in each case, if we think of what would be the limit to the sphere of attraction between the nebula and a ring just detached from it—for the attraction of the abandoned ring, and even of all those that were outside of it, would have very little influence in determining the line where gravitation and centrifugal force came to balance each other—but the data necessary for calculating what these would be are wanting. Even if they existed the calculations would become too complicated for our powers as the number of rings increased; and for our purpose it is really of very little importance where the divisions took place. The breadths of the rings would be practically the same, whether they were divided at the half distances between, or much nearer to, the outermost of two neighbouring planets; and although the extreme diameters of the consecutive residuary nebulæ would be somewhat greater, their densities and temperatures would not materially differ from those we shall find for them as we proceed in our operations. Their masses would be the same in all cases, which is the principal thing in which we are interested.

This premised, we shall first examine into the excessive heat attributed to the nebula, that being the first condition mentioned in our definition of the hypothesis.

The diameter of the sun being 867,000 miles, his volume is 341,238,000,000,000,000 cubic miles, and his density being 1·413 times that of water, his volume reduced to the density of water would be 482,169,000,000,000,000 cubic miles. Now, astronomers tell us that the whole of the planets, with their satellites and rings, do not form a mass of more than $\frac{1}{700}$th part of the mass of the sun. If, then, we add $\frac{1}{700}$th part to the above volume, we get a total volume, for the whole of the system, of 482,857,590,478,000,000 cubic miles at the density of water, which corresponds to a sphere of about 973,360 miles in diameter. On the other hand, the diameter of the orbit of Neptune being 5,588,000,000 miles, if we increase that diameter to 6,600,000,000 miles, so that the extreme boundary of the supposed nebula may be as far beyond his orbit, as half the distance between him and Uranus is within it, we shall still be far within the limit at which the process of separation from the nebula, of the matter out of which Neptune was made, must have begun. From these data we can form a very correct calculation of what the density—tenuity rather—of the nebula must have been. For, as the volumes of spheres are to each other as the cubes of their diameters, the cube 973,630 is easily found to be to the cube of 6,600,000,000, as 1 is to 311,754,100,720, or in other words, the density of the nebula turns out to have been $\frac{1}{311{,}754{,}100{,}720}$th part of density of the whole solar system reduced to that of water.

Carrying the comparison a little further, we find that as water is 773·395 times more dense than air, and 11,173·184 times more dense than hydrogen, the density of the nebula could not have been more than $\frac{1}{403{,}000{,}000}$th part that of air, and $\frac{1}{27{,}894{,}734}$th that of hydrogen. But, confining the comparison to air, as it suits our purpose better, we see that it would take 403,000,000 cubic feet of the nebula to be equal in mass to 1 cubic foot of air at atmospheric pressure; and that were we to expand this cubic foot of air to this number of times its volume, the space occupied by it would be as nearly in the state of absolute vacuum as could be imagined, far beyond what could be produced by any human means. Now, were heat a material, imponderable substance, as it was at one time supposed to be, we could conceive of its being piled up in any place in space in any desired quantity; but it has been demonstrated not only not to be a substance at all, but that its very existence cannot be detected or made manifest, unless it is introduced by some known means—friction, hammering, combustion—into a real material substance. Therefore, we must conclude that if it existed at all in the nebula, it must have been in a degree corresponding to the tenuity of the medium, and the air thermometer will tell us what the temperature must have been if we only choose to apply it.

Applying, then, this theory of the air thermometer, if we divide * 274° by 403,000,000—the number of times the density of the nebula was less than that of air—we get 0·00000068°, as the absolute temperature of the nebula, something very different to excessive heat, incandescence, firemist, or any other name that has been given to its supposed state. Furthermore, as a cubic foot of air weighs 565·04 grains, 403,000,000 divided by 565·04, which is equal to 713,223, would be the number of cubic feet of the space occupied by the nebula, corresponding to each grain of matter in the whole solar system, which would be equal to a cube of very nearly 90 feet to the side. And as the only means by which the nebula could acquire heat would be by collision with each other of the particles of matter of which it was composed ; to conceive that two particles weighing 1 grain each, butting each other from an average distance of 90 feet, could not only bring themselves, but all the space corresponding to both of them—which would be 1,426,446 cubic feet, *of what*?—up to the heat of incandescence, or excessive heat of any kind, is a thing which passes the wit of man. Consequently, neither by primitive piling up, nor by collisions among the particles, could there be any heat in the nebula at the dimensions we have specified, beyond what we have measured above.

Some people believe, at least they seem to say so, that meteors or meteorites colliding would knock gas out of each other, sufficient to fill up the empty space around them, and become incandescent, and so pile up heat in nebulæ sufficient to supply suns for any number of millions of years of expenditure. But they forget that gas is not a *nothing*. It possesses substance, matter, of some kind, however tenuous. Therefore, if the meteors knock matter out of each other in the form of gas, they must end by becoming gas themselves, and we come back to what we have said above ;

* Here we beg to state that in all our coming operations, we will use the Centigrade Scale for temperatures without adding C to each number specified, unless a different scale has to be referred to, in which case the distinctive of the scale shall be given in the usual way. This we do because it is the fashion, not because we think it possesses any advantage over any other scale, but rather the contrary. Perhaps we may have something more to say about scales after we have handled the Centigrade a little more than it has been our lot to do hitherto.

we have two grains, in weight, of gas abutting each other at an average distance of 30 yards, instead of two grains of granite or anything else, and things are not much improved thereby. And if we compare 30 yards with M. Faye's 3000, where are we?

The next thing to deal with is the formation of the planets.

SEPARATION OF RING FOR NEPTUNE.

When the nebula was 6,600,000,000 miles in diameter its volume would be 150,532,847,222^{18} * cubic miles, and we have just seen that its density must have been 311,754,100,720 times less than that of water, or 403,000,000 less than air, and its temperature 0·00000068° above absolute zero. On the other hand, we find from Table II. that the volume of Neptune and his satellite is 29,107,964,680,925 cubic miles at the density of water. Multiplying, therefore, this volume by 311,754,100,720 we get 9,074,530^{18} cubic miles as the volume of the ring for the formation of Neptune's system at the same density as the nebula. Then, subtracting this volume from 150,532,847,222^{18}, there remain 150,523,772,692^{18} cubic miles as the volume to which the nebula was reduced by the abandonment of the ring out of which Neptune and his satellite were formed.

Then the mean diameter of the orbit of Neptune being 5,588,000,000 miles, its circumference or length will be 17,555,261,000 miles, and if we divide the volume of his system as stated above, by this length, we get 516,912,620,000,000 square miles as the area of the cross section of the ring, which is equal to the area of a square of 22,735,123 miles to the side. Again, if we divide the circumference of the orbit by this length of side, we find that it is $\frac{1}{772 \cdot 165}$th part of it, and therefore about 28 minutes of arc. Also if we divide the diameter of the orbit by an arc of 22,735,123 miles in length, we find that it bears the proportion of 1 to 246 to the diameter of the orbit. Thus the cross section of the ring would bear the same ratio to its diameter that a ring of 1 foot square would

* The exponent 18 in 150,523,772,692^{18} means that 18 cyphers have to be added to complete the number. The same is the case with any other number and exponent of large quantities.

bear to a globe of 246 feet in diameter. Here we find it difficult to believe that by rotating a ball of 246 feet in diameter of cosmic matter, meteorites, or brickbats, we could detach from it, mechanically, by centrifugal force a ring of 1 foot square, and the same difficulty presents itself to us with respect to the nebula. We cannot conceive how a ring of that form could be separated by centrifugal force from a rotating nebula, and have therefore to suppose it to have had some different form, and to apply for that to the example of Saturn's rings—just the same as Laplace no doubt did. We cannot tell how the idea originated that the ring should be of the form we were looking for—perhaps it was naturally—but it seems to have been very general, and in some cases to have led to misconceptions. It is not difficult to show how a Saturnian or flat ring could be formed, but we shall have a better opportunity hereafter of doing so. We must try, nevertheless, to form some notion, however crude it may be, of what might be the thickness of a flat ring of the cross section and volume we have found for Neptune.

Let us suppose that the final separation of the ring took place somewhere near the half-distance between his orbit and that of Uranus, say, 2,290,000,000 miles from the centre of the nebula, the breadth of the ring would be the difference between the radius of the original nebula, i.e. 33,000,000,000 miles and the above sum, which is 1,010,000,000 miles. Then if we divide the area of the cross section of the ring by this breadth, that is, 516,912,620,000,000 by 1,010,000,000, we find that the thickness would be 511,794 miles; provided the ring did not contract from its outer edge inwards during the process of separation. This could not, of course, be the case, but, as we have no means of finding how much it would contract in that direction, we cannot assign any other breadth for it; and we shall proceed in the same manner in calculating the thicknesses of the rings for all the other planets as we go along. We can, however, make one small approach to greater accuracy. We shall see presently that the density of the ring would be increased threefold at its inner edge as compared with the outer during the process of separation, which would reduce its average thickness to somewhere about 341,196 miles at density of water, of course. The nebula remaining after Neptune's ring we may now call

THE URANIAN NEBULA.

The volume of the nebula after abandoning the ring for the system of Neptune was found to be $150,523,772,692^{18}$ cubic miles at its original density, but during the separation it has been condensed into a sphere of 4,580,000,000 miles in diameter, whose volume would be $50,303,255,814^{18}$ cubic miles ; so that if we divide the larger of these two volumes by the smaller, we find that the density of the Uranian nebula would be increased 2·9923 times, and therefore it would then be 311,754,100,720 divided by 2·9923, equal to 104,184,535,721 times less dense than water. Furthermore, if we compare it to the density of air, which we can do by dividing this last quantity by 773·395, we find it to have been 134.710,620 times less than that density; and if we apply the air thermometer to it, we shall find that its absolute temperature must have been 274 divided by $134,710,620 = 0·000002034°$ or $-273·9999796.°$

We can now separate the ring for the system of Uranus from the Uranian nebula, reduced as we have seen to 4,580,000,000 miles in diameter, volume of $50,303,255,814^{18}$ cubic miles, and density of 104,184,535,721 times less than water. Referring to Table II., we find the volume of the whole system of Uranus to have been 25,876,388,977,690 cubic miles at the density of water, but we have to multiply this volume by the new density of 104,184,535,721 times less than water in order to bring it to the same density as the nebula, which will make the volume of his system to be $2,695,918,851^{15}$ cubic miles at that density. Then, subtracting this volume from $50,303,255,814^{18}$, we find that the nebula has been reduced to $50,300,559,895,149^{15}$ cubic miles in volume.

Then the diameter of the orbit of Uranus being 3,566,766,000 miles, its circumference will be 11,205,352,065 miles, so that dividing the volume $2,695,918,851^{15}$ of his system by this length of circumference, the area of the cross section of the ring would be 240,592,061,166,666 square miles. If we now suppose the diameter of the nebula, after abandoning the ring for the whole system of Uranus, to have been 2,672,000,000 miles—dimension derived from nearly the half-distance between the orbits of Uranus and Saturn—we find that the breadth of the ring would be 954,000,000 miles, which would be the difference between the radii of the Uranian and Saturnian nebulæ, respectively 2,290,000,000 miles, and 1,336,000,000 miles ; so that if we divide the area of cross section of Uranus' ring or 240,592,070,232,288 square miles by this breadth we find the thickness of the ring to have been 252,193 miles. But the density of the inner edge of the ring would be 5·036 times more dense than the outer edge, for the same reason as in the case of the Neptunian ring, which would make the average thickness to have been about 100,553 miles.

Saturnian Nebula.

We have seen that the volume of the nebula after the separation of the ring for Uranus' system would be $50{,}300{,}559{,}859{,}149^{15}$ cubic miles, but as we have reduced the diameter of the Saturnian nebula to 2,672,000,000 miles, its volume would also be reduced, or condensed to $9{,}988{,}700^{21}$ cubic miles, so that dividing the larger volume by the smaller we find that its density must have been increased 5·036 fold. Then dividing 104,184.535,721 by 5·036, we see that the density would be reduced, or increased rather, to 20,689,000,000 times less than that of water. This can be easily found to be 26,750,876 times less than the density of air, and the air-thermometer would show that the absolute temperature of the Saturnian nebula must have been 0·000010242° or −273·99998976°.

We have just seen that the Saturnian nebula has been condensed to 2,672,000,000 miles in diameter, to volume of $9\,988{,}700^{21}$ cubic miles, and density of 20,689,000,000 times less than that of water. Then from Table II. we get the volume of the whole of the system of Saturn as 154 370,734,774,315 cubic miles at the density of water, and multiplying this by

20,689,000,000 will give $3,193,775,478^{15}$ as its volume at the same density as the nebula; and subtracting this from $9,988,700^{21}$ we find that the volume of the nebula had been reduced to $9,985,506,224,522^{15}$ cubic miles.

Then the diameter of the orbit of Saturn being 1,773,558,000 miles its circumference would be 5,571,809,813 miles in length, and if we divide the volume of his system, viz. $3,193,775,478^{15}$ cubic miles, by this length, we find the area of the cross section of the ring to have been 573,202,529,391,503 square miles. Now, supposing the diameter of the nebula, after abandoning the ring, to have contracted to 1,370,800,000 miles and radius consequently of 685,400,000 miles, the breadth of the ring would be 1,336,000,000 less 685,400,000 or 650,600,000 miles; and if we divide the area of the cross section of the ring, that is, 573,202,529,391,503 square miles, by this breadth, we get 881,037 miles for its thickness. But in the same way as before, the inner edge of the ring would be 7·4037 times more dense than the outer edge, which would reduce its average thickness to 238,000 miles.

JOVIAN NEBULA.

The volume of the nebula after separation of the ring for Saturn's system having been $9,985,506,224,522^{15}$ cubic miles, this volume has to be condensed into the volume of the Jovian nebula of 1,370,800,000 miles in diameter, which would be $1,348,720,186,335^{15}$ cubic miles. Then if we divide the first of these two volumes by the second, we find the density of the Jovian nebula to have been increased 7·4037 fold over the previous one. But the density of the Saturnian nebula was 20,689,000,000 times less than water, dividing which by 7·4037 makes the Jovian nebula to have been 2,794,417,420 times less dense than water. Dividing this by 773·395 we get a density for it of 3,613,182 times less than that of air, which corresponds to the absolute temperature of 0·00007583° or −273·99992417.

From the Jovian nebula of 1,370,800,000 miles in diameter, volume of $1,348,720,186,335^{15}$ cubic miles, and density of 2,794,417,420 times less than water, we have now to deduct the whole of the system of Jupiter, which, by Table No. II., is 479,368,921,317,000 cubic miles at density of water. Multiplying this by 2,794,417,420 we get the volume of $1{,}339{,}557{,}155^{15}$ cubic miles for his system at the same density as the nebula; therefore, substracting this amount from $1{,}348{,}720{,}186{,}335^{15}$ we get $1{,}347{,}380{,}629{,}180^{15}$ cubic miles as the volume to be condensed into the succeeding nebula which we shall call Asteroidal, the dimensions of which we can determine in the following manner, although only very approximately.

According to the nebula hypothesis, there must have been a ring detached from the nebula for the formation of the Asteroids, as well as the formation of the other planets. So, in order to be able to assign elements for that ring, corresponding to those we have found for the others, we shall suppose the whole of them to have been collected into one representative planet, at the mean distance from the centre of the nebula of 260,300,000 miles, more or less in the position denoted by the number 28 in Bode's Law; also its mass to have been one-fourth of that of the earth, or 367,792,000,000 cubic miles at density of water, which, in the opinion of probably most astronomers, is a considerably greater mass than would be made up by the whole of them put together—discovered and not yet discovered. With the above distance from the centre of the nebula, the divisionary line between the Jovian and the Asteroidal nebulæ would be 372,000,000 miles from the said centre, and the diameter of the latter 744,000,000 miles in consequence.

We know that some of the Asteroids move in their orbits beyond this supposed divisionary line, and it may be that when we come to determine the divisionary line between the supposed Asteroidal and the Martian nebulæ, some of them may revolve in their orbits nearer to Mars than that line, but that will not interfere in any way with our operations, because we are only dealing with the whole of them collected into one representative.

For finding the dimensions of the ring for Jupiter's system, we have the mean diameter of his orbit as 967,356,000 miles, which makes its circumference to be 3,039,045,610 miles in length. Therefore, dividing the volume of the ring as found above, viz. $1,339\,557,155^{15}$ cubic miles by this length, the area of its cross-section comes to be 440,782,188,524,000 square miles, which divided in turn by the breadth of 313,400,000—the difference between the radii of the Jovian and Asteroidal nebulæ, or 685,400,000 less 372,000,000—makes the thickness of the ring to have been 1,406,771 miles. But, as before, the inner edge of the ring had become 6·2484 times more dense than the outer edge, so that the average thickness would be only 450,282 miles.

ASTEROIDAL NEBULA.

The volume of the nebula after the separation of the ring for the system of Jupiter having been $1,347,380,629,180^{15}$ cubic miles, this volume has to be condensed into the volume of the Asteroidal nebula of 744,000,000 miles in diameter and consequently of volume of $215,634,925,373,133,820^{9}$ cubic miles. Then if we divide the first of these volumes by the second, we find the density to have been increased 6·2484 fold, as used above for the average thickness of Jupiter's ring. But the density of the Jovian nebula was 2,794,417,420 times less than water, dividing which by 6·2484 makes the Asteroidal nebula to have been 447,218,905 times less dense than water. This again divided by 773·395 makes it 578,254 times less dense than air, which will give us 0·00047384° as its absolute temperature—or the same as −273·99952616°.

Next, from the Asteroidal nebula 774,000,000 miles in diameter, volume of $215,634,925,373,133,820^{9}$ cubic miles, and density 447,218,905 times less than water, we have to deduct the volume of the whole of the system which in Table No. II. we have supposed to have been 367,792,000,000 cubic miles at density of water. Multiplying this by 447,218,905 we get the volume to have been $164,482,717,200^{9}$ cubic miles for the ring at the same density as the nebula ; so, deducting this quantity from $215,634,925,133,820^{9}$, we get $215,634,760,890,416,620^{9}$ cubic miles as the volume to which the nebula had been reduced by the separation of the ring.

For the dimensions of the ring we have the mean diameter of the orbit of the representative Asteroid as 520,600,000 miles, that is twice its distance from the centre of the nebula, which makes its circumference to be 1,635,516,960 miles in length. Dividing then the volume of the ring, which we found to have been 164,482,717,200^9 cubic miles by this length, the area of its cross-section must have been 100,569,251,938 square miles, which divided by the breadth of 171,000,000 miles—the difference between the radii of the Asteroidal and Martian nebula, namely 372,000,000 less 201,000,000—makes the thickness of the ring to have been 588 miles. But the inner having been 6·339 times more than the outer edge, as we shall see presently, the average thickness would be 185 miles.

Martian Nebula.

The volume of the last nebula after the separation of the ring for the Asteroids was found to have been 215,634,760,890,416,620^9 cubic miles, which had to be condensed into the volume of the Martian nebula of 402,000,000 miles in diameter, which would give a volume of 34,015,582,677,165,354^9 cubic miles. Dividing then, the larger of these volumes by the smaller, we find that the density of the Martian nebula had been increased 6·339 times by the condensation. But we found the density of the Asteroidal nebula to have been 447,218,905 times less dense than water, dividing which by 6·339 makes the Martian nebula to have been 70,547,110 times less dense than water. This divided again by 773·395 makes it 91,259 times less dense than air, and consequently its absolute temperature to have been 0·00300243° or −273·99699757°.

From the Martian nebula of 402,000,000 miles in diameter, volume 34,015,582,677,165,354^9 cubic miles, and density 70,547,110 times less than water, we have to deduct the volume of his ring, which by Table II., was estimated at 160,728,460,000 cubic miles at density of water. Multiplying this by 70,547,110 we find its volume to be 11,338,927,154^9 cubic miles at the same density as the nebula, deducting which from its whole volume we get $34{,}015{,}571{,}338{,}237{,}200^9$ cubic miles as the volume after the separation of the ring.

For finding the dimensions of the ring we have 283,300,000 miles as the mean diameter of the orbit of Mars, which makes its circumference 890,015,280 miles in length. Then dividing the volume of the ring $11{,}338{,}927{,}154^9$ cubic miles by this length, the area of its cross-section comes to be 12,740,148,859 square miles, which, divided by the breadth of 83,690,000 miles—that is one-half of the difference between the diameters of the Martian and Earth nebula, respectively 402,000,000 and 234,620,000 miles—makes the thickness of the ring to have been 152 miles. But as before, the inner having become through condensation, 5·0302 times more dense than the outer edge, the average thickness would be 61 miles.

Earth Nebula.

As the volume of the nebula was $34{,}015{,}571{,}338{,}237{,}200^9$ cubic miles after the separation of the ring for Mars, we have to condense it into the volume of the earth nebula, which at 234,620,000 miles in diameter would be $6{,}762{,}303{,}076{,}923{,}031^9$ cubic miles. Dividing the larger of these volumes by the smaller we find that the density of the nebula has been increased 5·0302 times, as employed above. But we found the density of the Martian nebula to have been 70,547,110 times less than that of water, dividing which by 5·0302 makes the earth nebula to have been 14,024,781 times less dense than water. Dividing this again by 773·395 we find it to have been 18,134 times less dense than air, and 274° divided by this density of air—the same as in all the respective cases—gives 0·0151097° as the absolute temperature of the nebula and corresponds to -273 9848903°.

From the earth nebula 234,620,000 miles in diameter, $6{,}762{,}303{,}076{,}923{,}031^9$ cubic miles in volume, and 14,024 781 times less dense than water, we have to subtract the volume of the ring of the earth's system, which, in Table II., appears as 1,489,310,236,000 cubic miles at density of water. Multiplying this by 14,024,781 we find it to have been $20{,}887{,}249{,}553^9$

cubic miles at the same density as the nebula. And subtracting this quantity from 6,762,303,076.923,031^{9}, we get 6,762,282,189,673,478^{9} cubic miles for the volume of the previous nebula after the separation of the ring for the system of the earth.

For finding the dimensions of the ring we have 185.930,000 miles for the mean diameter of the Earth's orbit, which makes the circumference 584,117,688 miles in length, and dividing the volume of the ring for the system, which was found to be 20,887,249,553^{9} cubic miles, by this length, the area of its cross section comes to be 35,760,344,109 square miles, which divided by the breadth of 37,205,000 miles—that is one-half of the difference between the diameters of the Earth and Venus nebulæ, respectively 234,620,000 and 160,210,000 miles—makes the thickness of the ring to have been 961 miles. But the inner will presently be seen to have been 3·141 times more dense than the outer edge when its separation was completed, so that the average thickness would be 612 miles.

Venus Nebula.

As the volume of the nebula was 6,762,282,189,673,478^{9} cubic miles after the separation of the ring for the system of the Earth, we have to condense it into the volume of the Venus nebula, which at 160,210,000 miles in diameter would be 2,153,120,792,079,208^{9} cubic miles. Then dividing the larger of these two volumes by the smaller, we find that the density of the Venus nebula had been increased to 3·141 times what that of the Earth nebula was. But we found the density of that nebula to have been 14,024,781 times less than that of water, dividing which by 3·141 makes the Venus nebula to have been 4.465,512 times less dense than water. Dividing this again by 773·395 we find it to have been 5,774 times less dense than air, which would make its absolute temperature to have been 0·04745486°, which corresponds to -273·9525459°.

From the Venus nebula of 160,210,000 miles in diameter, volume 2,153,120,792,079,207,921^{6} cubic miles, and density 4,465.512 times less than that of water, we have now to deduct the volume of her ring, which by Table **II.** is 1,131,960,000,000 cubic miles at the density of water. Multiplying this volume by 4,465,512 we find the volume of the ring to have been 5,054,780,604,651^6 cubic miles at the same density as the nebula, and subtracting this amount from 2,153,120,792,079,207,921^6 we get 2,153,115,737,298,603^6 cubic miles for the volume to be condensed into the nebula following.

To find the dimensions of the ring we have 134,490,000 miles for the diameter of the orbit of Venus, which makes its circumference 422,513,784 miles in length. Then dividing the volume of the ring, i.e. 5,054,780,604,651^6 cubic miles by this length, the area of its cross-section comes to be 11,963,821,788 square miles, which, divided by the breadth of 28,489,000 miles—that is one-half of the difference between the diameters of the Venus and Mercurian nebulæ, respectively 160,210,000 and 103,232,000 miles—makes the thickness of the ring to have been 420 miles. But the inner edge having become, in the process of separation, 3·738 times more dense than the outer one (see below) the average thickness would be reduced to 225 miles.

MERCURIAN NEBULA.

As the volume of the nebula was 2,153,115,737,298,603,270^6 cubic miles after the separation of the ring for Venus, we have to condense it into the volume of the Mercurian nebula, which at 103,232,000 miles in diameter would be 576,026,613,333,333,333^6 cubic miles. Then, dividing the larger of these two volumes by the smaller, we find that the density of the Mercurian nebula must have been increased 3·738 fold over that of its predecessor. But we find the density of the Venus nebula to have been 4,465 512 times less than water, dividing which by 3·738 makes the Mercurian nebula to have been 1,194,666 times less dense than water. Dividing again this density by 773·395 we find it to have been 1545 times less than air, and 274° divided by this air density gives 0·1773463 as its absolute temperature, which corresponds to −273·8226537°.

From the Mercurian nebula 103,232,000 miles in diameter, volume of 576,026,613,333,333,333[6] cubic miles, and density of 1,194,666 times less than water, we have to deduct the volume of his ring, which by Table II. is 92,735,000,000 cubic miles at density of water. Multiplying this volume by 1,194.666 makes the ring to have been 110,787,355,300[6] cubic miles in volume at the density of the nebula, and subtracting this amount from 576,026,613,333,333,333[6], we get 576,026,502,545,978,033[6] cubic miles for the volume to be condensed into the nebula following.

To find the dimensions of the ring we have 71,974,000 miles for the mean diameter of the orbit of Mercury, which makes its circumference 226,113,518 miles in length. Then dividing the volume of his ring, i.e. 110,787,355,300[6] cubic miles, as above, by this length, the area of its cross-section comes to be 489,963,459 square miles. Here we have to determine the breadth of the ring in a new way, that is empirically. Seeing that the breadth of the ring for the earth's system was 37,205,000 and of that for Venus 28,489,000 miles, we shall assume 20,000,000 miles for the breadth of the ring for Mercury. This will make the residuary, now the Solar nebula, to have been 31,616,000 miles in radius and 63,232,000 miles in diameter. Returning now to the area of the cross-section of the ring, that is, 489,963,459 square miles, and dividing it by the assumed breadth 20,000,000 miles, makes the thickness of the ring to have been 25 miles. But, as before, its inner edge having become 4·354 times more dense than the outer one during the process of separation (see below) the average thickness must have been only 11 miles.

Solar Nebula.

Lastly, as the volume of the nebula was

576,026,502,545,978,033[6]

cubic miles after the separation of the ring for Mercury, we have to condense it into the volume of the Solar nebula, which at 63,232,000 miles in diameter would be

132,376,310,975,609,756[6]

cubic miles. Then dividing the first of these two volumes by the second, we find that its density must have been increased 4·3514 fold. But we found that the density of the Mercurian nebula was 1,194,656 times less than that of water, dividing which by 4·3514 makes the Solar nebula to have been 274,546 times less dense than water. Dividing this in turn by 773·395 shows it to have been 355 times less dense than air, and, still further, dividing 274° by this air density makes its absolute temperature to have been 0·7718585° equal to −273·2281415°.

We might conclude our analysis here, but it will be more convenient to carry our calculations a few steps further, to save the additional trouble that might be occasioned by having to return to them later on.

First we shall condense the Solar nebula to 211,911 times less dense than water, and therefore 274 times less dense than air, which we may note will increase its density 1·2956 times. This supposed to be done, its diameter would be 58,002,920 miles, its volume $102{,}176{,}129{,}412^{12}$ cubic miles, and its density $\frac{1}{274}$th of an atmosphere—about *one-ninth* inch of mercury—which would, in consequence, make its absolute mean heat equal to *one degree* of the ordinary Centigrade scale, or, in another way of expressing it, equal to −273°.

Second. Let us condense this same nebula to 773·395 times less dense than water, and consequently to the density of air at atmospheric pressure, then its diameter will be 8,930,309 miles, volume $372{,}905{,}560{,}345^{9}$ cubic miles, and the mean heat 0°, or the heat of freezing water—which by some unexplained process of thought has hitherto been considered to be 274° of absolute *temperature.*

Third. By again condensing the Solar nebula to the density of water, corresponding to a pressure of more than 773 atmospheres, its diameter becomes 972,285 miles, its volume $482{,}167^{12}$ cubic miles, and mean heat 775°, including the 2° acquired in condensing it to the pressure of 1 atmosphere, as is plainly shown in Table III.

Before going any further we must enter into a digression to examine into the process of thought by which the absolute zero of heat has come to be called the absolute zero of temperature, and absolute temperature to be so many degrees of negative—less than 0° or nothing—heat counted from the lower or wrong end, to be called positive absolute temperature; thus making heat and temperature appear to be two very different things, without giving any explanation of what is the difference between them.

Science has, as it were, gone down a stair of 274 steps carrying along with it the laws of gases, and has found, most legitimately, with their assistance the total absence of even negative heat at the bottom of it; and, leaving these laws there, has jumped up to the top of the stair, thinking that it carried along with it 274° of absolute heat, which it now calls temperature; instead of bringing the said laws up with it and verifying, if not at every step at least at intervals, how much it brought up with it of what it had taken down. Had it done so it would have found that at the top of the stair it had got what was equal to only 2° of positive heat as measured by the Centigrade scale, as has been shown above, which might be called temperature, but that would not mend matters. Science seems to have forgotten, for the time being at least, all about the laws of gases; it had got something which it thought would enable it to mount much higher, and was satisfied. It will not be difficult to do away with the confusion of thought that is thus shown to have occurred.

The laws of gases are founded upon the fact that in gases there is a necessary interdependence between heat and pressure, and the starting points adopted by science for calculating this interdependence in them are 0° of heat and 1 atmosphere of pressure at 0° of heat. Obeying these laws, we have argued, from the beginning of our operations, that heat requires something to hold it in, and that the nebula from which the Solar system was formed—if it was so formed—could only contain heat in proportion to its density; that is being a gas, or vapour in the form of a gas, it could not contain, i.e. hold in it, more than 2° of positive heat when its density was equal to the pressure belonging to 1 atmosphere of a gas; all as shown in the most irrefragable manner in this chapter and in the accompanying Table III.

A gas can be easily compressed in a close vessel to a pressure of 100 atmospheres, which would enable it to hold 100° of heat due to that compression; in fact, were it compressed to that degree by a piston in a cylinder, without any loss of heat, it would be raised to that heat by that act alone, but that would raise it to only 102° instead of 374° of what is called absolute temperature according to present usage; because as a gas it could not hold any more heat at that pressure. It is, therefore, evident that this *usage* has not been derived from the laws of gases. Neither has it been derived from the other two states of liquid and solid to which all gases can be reduced, as can be very easily demonstrated.

To cool steam at atmospheric pressure from its gaseous to its liquid state 519° of heat of one kind and another—as measured by the Centigrade thermometer—have to be abstracted from it, which leaves the liquid at its boiling point of 100°—a quantity that has been arbitrarily adopted to mark the difference between the freezing and boiling points of this liquid. In order, after this, to reduce the liquid, now water, to the freezing, or what is called 0° of heat, these 100 degrees of heat have to be extracted from it, which is not very difficult to do because the heat put into it arbitrarily can be extracted from it; but if it is now wanted to change the steam from its liquid to its solid state, the work, or operation assumes a very different character, because heat cannot be extracted from a substance which contains none at all. It is well known that 80° of heat are required to change one pound of ice at 0° into a pound of water also at 0° of heat; but it is equally well known that 80° of heat cannot be taken out of the pound of water which has none in it; how then, is the water to be changed into ice?

Even in cooling water to 0° it has to be put into a bath of some kind, either of cold water or some cold mixture of other substances at least as cold; because, otherwise, extraneous heat from any source might find its way into it, and prevent it from cooling down to zero of heat. In the same manner, to change the water into its solid state of ice it has to be put into a similar bath, not to extract heat from it, because it has not any to extract, but to prevent extraneous heat from getting into it. This being the case, it is evident that if water is put into a bath at what is called -1° of heat, or even a fraction of that amount, it will be converted into ice though very gradually, by keeping extraneous heat from getting to it to sustain the collisions, or vibrations, of its constituent atoms necessary to maintain it in its liquid state. All for the very same reason why a stone, a piece of metal, or of anything assumes the same degree of heat, or absence of heat, as the medium by which it is surrounded; be it derived from sun-heat, earth-heat, or heat produced chemically or mechanically, and is not cooled down to a lower degree than the surrounding bath, be it what it may.

The heat required to change a solid into a liquid is called *latent heat*, which in the case of ice and water may be a fraction of -1° or -80°, or *minus* almost anything according to the time it is necessary for it to act; so that no quantity of what is called absolute temperature can be ascribed to ice without the element time being involved in it. The absolute temperature of water and ice, just changing from freezing to frozen, might be counted as the same, seeing that a fraction of a degree of heat may make all the difference between them; but no fixed absolute temperature can be applied to ice, as it, in conjunction with all solid bodies, may have any degree of absolute temperature between its melting point and the absolute zero of heat, as far as is at present known. The same, of course, must be the case with any gas or vapour, or nebulous matter changed into its liquid and then solid state; and this fact enables us to go a little further.

We have seen that what, according to present usage, is called the absolute temperature of solid hydrogen may be anything between -257° and -274° of heat, that is, between the absolute temperature of 0° and 17°, which, of course, is no measure at all; and, therefore, absolute temperature can only be looked upon as a conventional term, which, when added to positive Centigrade, or other, heat, conveys no clear idea to the mind, as it must always be mixed up with the concomitant idea of latent heat and its time of action. This leads us to think of what remains in the vessel, in which pure hydrogen has been changed into its liquid and then solid state, after these operations have been performed ; and our first conclusion comes to be that there can be nothing in it but a small piece of solid hydrogen ; but from the limited accounts we have seen of these operations, there does appear to be something remaining, because it seems that by it the degree of negative heat in the vessel can be measured. What that remaining something may be can hardly be anything but a matter of conjecture. The first and most probable idea that occurs is that it may be some lighter gas mixed with the pure (?) hydrogen that was put into the vessel ; the next is that it may be the vapour of solid hydrogen ; and the last refuge for speculation is that it may be radiant matter, whatever that may turn out to be. At one time it was supposed to be impurities mixed with the gases operated upon, which in the case of common air, were found to be removed to a certain extent by means of absorbents ; but the numerous components of common air discovered since that time, have gone far to throw light upon that supposition, and we are thus led to think of what a true gas really is. But we are not yet prepared to follow up this thought.

This is not an inappropriate place to say that when we adopted the Centigrade scale for our work, we thought that a special thermometer, decimal throughout and consequently more handy, might be arranged for science alone, leaving every man the free use of whatever scale he liked best ; but our experience acquired in this chapter put an end to that thought, and has left us totally unable to see how any decimal scale can be contrived, which will start from absolute zero of heat and will admit of any combination with any existing scale, or will assist humanity in any of its operations in connection with heat and temperature, whichever science may choose to call it. We therefore see that no known thermometer scale is superior to another, and end where we began by saying that the Centigrade is the fashionable one at the present time. It is decimal as far as boiling water and resulting steam are concerned, but all the world is not boiling water ; even steam has to be complicated with latent heat.

TABLE III.—ABSTRACT OF MEASUREMENTS, ETC., RESULTIN

Nebulæ. Name.	Nebulæ. Diameter in miles.	Explanations.	Volume of the Mass of each Separate System at Density of Water in cubic miles.	Times less Dense than Water.	Increase of Density in times.
Original or Neptunian	6,600,000,000	.. ..	.. ..	311,754,100,720	..
Ditto . .	..	Volume of Neptune's Ring . .	29,107,964,680,925	311,754,100,720	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Uranian .	4,580,000,000	Condensed from Neptunian Nebula	.. ..	..	2·9923
Ditto . .	..	Volume of Uranus' Ring . .	25,876,388,977,000	104,184,535,721	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Saturnian .	2,672,000,000	Condensed from Uranian Nebula .	.. ..	..	5·0357
Ditto . .	..	Volume of Saturn's Ring . .	154,370,734,774,315	20,689,000,000	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Jovian . .	1,370,800,000	Condensed from Saturnian Nebula .	.. ..	..	7·4037
Ditto . .	..	Volume of Jupiter's Ring . .	479,368,921,317,000	2,794,417,420	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Asteroidal .	744,000,000	Condensed from Jovian Nebula .	.. ..	..	6·2484
Ditto . .	..	Volume of Asteroidal Ring . .	367,792,000,000	447,218,905	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Martian .	402,000,000	Condensed from Asteroidal Nebula .	.. ..	..	6·3392
Ditto . .	..	Volume of Martian Ring . .	160,728,460,000	70,547,110	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Earth . .	234,620,000	Condensed from Martian Nebula .	.. ..	..	5·0302
Ditto . .	..	Volume of Earth Ring . . .	1,489,310,236,000	14,024,781	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Venus . .	160,210,000	Condensed from Earth Nebula .	.. ..	..	3·1410
Ditto . .	..	Volume of Venus Ring . . .	1,131,960,000,000	4,465,512	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Mercurian .	103,232,000	Condensed from Venus Nebula .	.. ..	..	3·7379
Ditto . .	..	Volume of Mercurian Ring . .	92,735,000,000	1,194,666	..
Ditto . .	..	Volume of Nebula less ring . .	.. ..	..	..
Solar . .	63,232,000	Condensed from Mercurian Nebula.	.. ..	274,546	4·3514
Ditto . .	58,002,920	Volume at $\frac{1}{374}$ of an atmosphere .	.. ..	211,911	1·2956
Ditto . .	8,930,309	Volume at density of one atmosphere	.. ..	..	274·000
Ditto . .	972,895	Volume at density of water . .	.. ..	..	773·395

Volumes at Densities of Respective Nebulæ in cubic miles.	Times less Dense than Air.	Absolute Temperature. (Degrees.)	At Density of Water.			At Air Density.		
			Dimensions of Rings.			Space to Grain of Matter.		
			Breadth in miles.	Thickness in miles.	Average Thickness in miles	Cubic Feet.	Side of Cube in Feet.	Inches
150,532,847,222,000,000,000,000,000,000	403,000,000	0·00000068	..	..	..	713,223	89·327	
9,074,530,000,000,000,000,000,000	..	..	1,010,000,000	511,794	341,196			
150,523,772,692,000,000,000,000,000,000								
50,303,255,814,000,000,000,000,000,000								
2,695,918,851,000,000,000,000,000	134,710 620	0·000002034	954,000,000	252,193	100,553	238,357	61·994	
50,300,559,895,149,000,000,000,000,000								
9,988,700,000,000,000,000,000,000								
3,193,775,478,000,000,000,000,000	26,750,876	0·00001024	650,600,000	881,037	238,000	47,313	36·168	
9,985,506,224,522,000,000,000,000								
1,348,720,186,335,000,000,000,000								
1,339,557,155,000,000,000,000,000	3,613,182	0·00007583	313,400,000	1,406,771	450,282	6,303	18·472	
1,347,380,629,180,000,000,000,000								
215,634,925,373,133,820,000,000,000								
164,482.717,200,000,000,000	578,254	0·00047384	171,000,000	588	185	1,023	10·075	
215,634,760,890,416,620,000,000,000								
34,015,582,677,165,354,000,000,000								
11,338,927,154,000,000,000	91,259	0·00300244	83,690,000	152	61	161	5·445	
34,015,571,338,237,200,000,000,000								
6,762,303,076,923,031,000,000,000								
20,887,249,553,000,000,000	18,134	0·0151097	37,205,000	961	612	32	3·178	
6,762,282,189,673,478,000,000,000								
2,153,120,792,079,207,921,000,000								
5,054,780,604,651,000,000	5,774	0·047454	28,489,000	420	225	10·2	2·170	
2,153,115,737,298,603,270,000,000								
576,026,613,333,333,333,000,000								
110,787,355,300,000,000	1,545	0·1773463	20,000,000	25	11	2·734	1·398	
576,026,502,545,978,033,000,000								
132,376,310,975,609,756,000,000	355	0·771831	..	..	..	0·6283	0·8565	10·28
102,176,129,412,000,000,000,000	274	0·99635	..	..	..	0·4848	0·7856	9·43
372,905,560,345,000,000,000	0	2·0000	..	..	..	0·00177	0·121	1·452

Returning now to page 84, we see that the volume of the sun alone was considered to be $482,169^{12}$ cubic miles, which corresponds to a diameter of 972,869 miles. Comparing this with the volume $482,167^{12}$ cubic miles, see page 99, left after all the members of the Solar system have been separated from the original nebula, we find that there is a remainder of 2,000,000,000,000 cubic miles *less* than we ought to have. But it will be remembered that we added only $\frac{1}{700}$th part to the mass of the sun for the mass of the whole Solar system, whereas it will be seen, by referring to Table II., that we ought to have added $\frac{1}{696 \cdot 86}$th part. Had we done so the sphere containing the whole Solar system at the density of water would have been 973,361·31 miles in diameter with volume of $482,860,744^{9}$ cubic miles, which would have added 3,153,681,000,000 cubic miles to the volume we started with, and would have left us with 1,375.903,430,000 cubic miles *more* than we ought to have had. Besides, for the sake of round numbers, we made the diameter of the nebula containing the whole Solar system, at the density of water, to be 973,360 instead of 973,359·208 miles, and thereby really added more to the original volume than we should have; so that the defects in accuracy at the beginning of our work partially counterbalanced each other, which accounts so far for the difference noted at the end not being much more than half of what it should have been. Taking all this into consideration, and the really insignificant magnitudes of the differences that would result from the corrections that could be made, we have not thought it necessary to reform the whole of our calculations. Besides, the data we have been working upon are not so absolutely exact as to insure us that we should get nearer to the truth by making the revision. The whole error would be much more than obliterated were we to apply 5·67 instead of 5·66 for the mean density of the earth to the debit side of the sun's account.

To simply describe arithmetical operations conveys no really satisfactory meaning to the mind; of working them out in full there is no end; and to partially represent them as we have done in these pages, although showing how the results are arrived at, still leaves them so mixed up together that it is difficult to compare them with each other, and to note the sequences from the beginning to the end of the whole operation. For these reasons we have compiled Table III., where the whole of the principal and most important data, and results from them, may be followed out and examined.

We may now say that we have taken our nebula to pieces, with the exception of the parts belonging to the satellites of those planets which have them; which would only be a tiresome repetition of what we have done for each principal member of the system, provided we had the necessary data, which we have not; and have thus acquired a certain amount of knowledge of the primitive conditions of each one of them. But we have still to examine into and draw conclusions from what we have seen and learned during the operation; which in some points, differ very much from our notions, formed from what we had previously read on the subject.

CHAPTER VI.

PAGE

108 Analysis continued. Excessive heat of nebula involved condensation only at the surface. Proof that this was Laplace's idea.

109 Noteworthy that some astronomers still believe in excessive heat.

110 Interdependence of temperature and pressure in gases and vapours. Collisions of atoms the source of heat.

110 Conditions on which a nebula can be incandescent. Sir Robert Ball.

112 No proper explanation yet given of incandescent or glowing gas.

115 How matter was thrown off, or abandoned by the Jovian nebula.

116 Division into rings of matter thrown off determined during contraction.

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120 Form gradually assumed by nebulæ. Cause of Saturn's square-shouldered appearance.

120 A lens-shaped nebula could not be formed by surface condensation.

121 Retrograde rotary motion of Neptune and Uranus, and revolution of their satellites recognised by Laplace as possible.

123 Satellites of Mars. Rapid revolution of inner one may be accounted for.

124 Laplace's proportion of 4000 millions not reduced but enormously increased by discoveries of this century.

ANALYSIS OF THE NEBULAR HYPOTHESIS—*continued.*

WHEN Laplace elaborated his hypothesis, heat was considered to be an imponderable material substance, and continued to be thought of as such—though perhaps not altogether believed to be so—for somewhere about half a century afterwards ; so that it cannot be wondered at that he thought the nebula could have been endowed with excessive heat, more especially as it was looked upon as imponderable, and could in no way have any effect on the mass of the nebula. He only accepted the idea that was common to almost all astronomers of his time, that nebulæ were masses of cosmic matter of extreme tenuity but self-luminous, and consequently possessed of intense heat; they saw the sun gave light and felt its heat, and very naturally thought the nebula must be hot also. Without this idea he could not have formed the hypothesis at all, because he could not have conceived that the condensation of the nebula could only take place at its surface, or, as he terms it, "in the atmosphere of the sun," as most assuredly would be the case with an excessively hot body. And in order that there may be no doubt about this being his idea, we quote his own words as guaranteed by M. Faye in "L'Origine du Monde": "La considération des mouvements planétaires nous conduit donc à penser qu'en vertu d'une chaleur excessive l'atmosphère du soleil s'est primitivement étendu au delà des orbes de toutes les planètes, et qu'elle s'est reserrée successivement jusqu'à ses limites actuelles." And again: "Mais comment l'atmosphère solaire a-t-elle déterminé les mouvements de rotation et de révolution des planètes et des satellites? Si ces corps avaient pénétré profondément dans cette atmosphère, sa résistance les aurait fait tomber sur le soleil. On peut donc conjecturer que les planètes ont été formées à ses limites successives par la condensation des zones de vapeurs qu'elle à dû, en se refroidissant, abandonner dans le plan de son équateur." Proceeding on these ideas Laplace was quite in order and logical in conceiving that successive rings could be abandoned by the hot nebula, through the centrifugal force of rotation, for the formation of planets, more or less just in the way we have separated them. Having obtained his end quite legitimately, as he thought, in this way, he had no occasion to look any deeper into the affair, and consequently was not under the necessity of taking any thought of what the interior construction of the nebula might be, any more than so many others have not done since his day.

That he should have conceived the nebula to have been endowed with intense heat was, as we have already said, a natural consequence of the mistaken notions of the nature of heat at that period; but that so many astronomers should, up to the present day, think that the nebula must have been intensely hot, even to the degree required to dissociate the meteorites of which they conceive it to have consisted, seems to us to be almost inconceivable. We believe we have shown abundantly plainly, that there could have been almost no heat in the primitive nebula, because there was hardly any cosmic matter to hold it in. We have given as proof of this the laws of gases recognised and accepted by every scientist, according to which a gas cannot contain a stated amount of heat except it be at a pressure corresponding to that temperature, that is, unless it is subjected to conditions foreign to its natural state. Therefore we must either persist in maintaining that there was almost no heat in the original nebula, or we must throw the laws of gases to the winds, for they all depend one upon another. There may be nebulæ possessed of very high temperature, that of incandescence for example, but certainly the nebula out of which the solar system was made, could not have contained more heat than what we have shown it had at the various stages through which we have carried it. If there be nebulæ at the temperature of incandescence, they must be possessed of densities, or pressures, corresponding to that temperature. A few pages back we have spoken of the impossibility of two grains of matter 90 feet apart, raising, by mutual collisions, their temperature and that of the space occupied by each to the temperature of incandescence, and if we now substitute for them meteorites of a pound weight each, the space occupied by each of them will be a cube of 1670 feet to the side, which does not help us in any way to believe that the spaces occupied by them could be heated up by their collisions, so as to shine with the temperature of incandescence. So we get no help from meteorites.

Some people evidently seem to think that nebulæ can be incandescent and give the spectrum of incandescent gas, without their density or pressure being increased to the corresponding degree. Sir Robert Ball seems to be one of them, though at the same time he appears to be not altogether sure of it. When discussing the self-luminosity of the nebula in Orion, in his "Story of the Heavens," Ed. 1890, p. 465, he says: "We have, fortunately, one or two very interesting observations on this point. On a particularly fine night, when the speculum of the great six-foot telescope of Parsonstown was in its finest order, the skilled eye of the late Earl of Rosse and of his assistant, Mr. Stoney, detected in the densest part of the nebula myriads of minute stars, which had never before been recognised by human eye. Unquestionably the commingled rays of these stars contribute not a little to the brilliancy of the nebula, but there still remains the question as to whether the entire luminosity of the great nebula can be explained, or whether the light thereof may not partly arise from some other source. The question is one which must necessarily be forced on the attention of any observer who has ever enjoyed the privilege of viewing the great nebula through a telescope of power really adequate to render justice to its beauty. It seems impossible to believe that the bluish light of such delicately graduated shades has really arisen merely from stellar points. The object is so soft and so continuous—might we not almost say ghost-like?—that it is impossible not to believe that we are really looking at some gaseous matter."

Here we see that his own belief about the matter is not very firm. He admits that the stars contribute not a little to the brilliancy of the nebula, and the most he can say in favour of its shining with its own light is, that it seems impossible to believe that the light has arisen merely from stellar points. He then goes on to show how the self-luminosity may be explained, as follows:—

"But here a difficulty may be suggested. The nebula is a luminous body, but ordinary gas is invisible. We do not see the gases which surround us and form the atmosphere in which we live. How, then, if the nebula consisted merely of gaseous matter, would we see it shining on the far distant heavens? A well-known experiment will at once explain this difficulty. We take a tube containing a very small quantity of some gas: for example hydrogen; this gas is usually invisible; no one could tell that there is any gas in the tube, or still less could the kind of gas be known; but pour a stream of electricity through the tube, and instantly the gas begins to glow with a violet light. What has the electricity done for us in this experiment? Its sole effect has been to heat the gas. It is, indeed, merely a convenient means of heating the gas and making it glow. It is not the electricity which we see, it is rather the gas heated by the electricity. We infer, then, that if the gas be heated it becomes luminous. The gas does not burn in the ordinary sense of the word; no chemical change has taken place. The tube contains exactly the same amount of hydrogen after the experiment that it did before. It glows with the heat just as red-hot iron glows. If, then, we could believe that in the great nebula of Orion there were vast volumes of rarefied gas in the same physical condition as the gas in the tube while the electricity was passing, then we should expect to find that this gas would actually glow."

There is a great deal to be said about this explanation. We presume that a very small quantity of hydrogen gas means that it was considerably below atmospheric pressure. Even so we admit that by introducing sufficient heat into the tube by means of electricity or otherwise, the gas could be raised to the temperature of incandescence, but its pressure would, at the same time, be increased to the corresponding force measured in atmospheres; and we also admit that when the gas was allowed to cool down to its original temperature, the same quantity of hydrogen would be found in the tube; but how about the tube? When the gas came to be at the temperature of incandescence the tube would be the same, or very soon raised to it, and being made of glass would be sufficiently plastic to be distorted, or even burst by the pressure within, probably even before the gas reached the temperature of incandescence. We must not forget that the first appearance of incandescence begins with red heat whose temperature is not far from 500° in daylight, and that white heat rises to above 1000°. If the experiment was made in an almost capillary tube, sufficiently thick to prevent accidents, then it might appear to prove a foregone conclusion, but nothing else; it might keep the idea of pressure out of sight, but it could not prove that the gas inside was in a rarefied state when incandescent. That the gas glowed the same as a red-hot bar of iron has not been shown. The gas had to be shut up in a tube to make it glow, but the bar of iron could glow outside of the tube. Could a streak of hydrogen be put into a furnace along with a bar of iron and heated to incandescence by its side, there might be some fair comparison between them, as long as they were in the furnace together, but the moment they were taken out the glow would disappear from the gas, whereas the iron would glow for some time. On the other hand we might *say* that a stream of incandescent gas might be made to heat a bar of iron in an oven to its own temperature, but the moment the stream of gas and the iron bar were removed from the oven, the former would disappear at once and the latter would continue to glow, simply because it was dense enough to contain a very considerable supply of heat compared to what the gas could, or rather, because the pressure of the gas, even did it correspond to the temperature, would disappear at once and the heat with it. So it is not always safe to *say* things. But it is quite safe to say that no gas—or substance such as we are accustomed to look upon as gas—can abide in a state of incandescence, and merely glow, unless its pressure, or density, corresponds to the temperature of incandescence; which for red heat (in the dark) would be $370^{\circ} = 2 \cdot 35$ atmospheres, and for white heat at $1000^{\circ} = 4 \cdot 65$ atmospheres, above absolute zero of pressure in both cases. And also, that if the self-luminosity of a nebula arises from incandescent gas, the pressure in the gas of that nebula must be somewhere between 2 and 5 atmospheres above absolute zero of pressure. Now we have shown, at page 85, that the density and pressure in the solar nebula, at the stage there specified, could not have been more than the 403 millionth part of those of our atmosphere, and consequently were justified in asserting that in it there could be almost no heat whatever.

We have just been speaking of a streak of gas and a bar of iron being heated in an oven to a red or white heat side by side, but everybody knows that this could not be done; but everybody has not thought of why it could not be done, otherwise Sir Robert Ball would not have favoured us with his laboratory experiment of a streak, or remnant, of hydrogen in a glass tube. We know that a plate, or bar, of iron can be heated up to the temperature of incandescence in an oven, but it has never occurred to anyone, who has seen the thing done, that the gas, air, or vapour which heats them must be at a pressure corresponding to that temperature. Multitudes of people may have thought of how the thing is done, but apparently very few have thought that it is not the gaseous part of the current of heated matter introduced into the oven, that heats it and the metal in it, but the solid part which is the distinctive and most important part of the constituents of the current. The solid part of the matter—let it be gas or any other element—is heated to incandescence in some furnace and carried along by the gaseous part—that is the *stuff* that fills the empty spaces between the solid molecules—to give it out to the oven and iron. We are not sure that the gaseous part even glows. We see plainly enough that the walls of the oven glow, but with respect to the gas, or carrying agent, we are inclined to think that it rather dims the glow of the oven and iron than otherwise. In passing, we say it is not unreasonable to suppose that the solid matter which contained the heat till it was given out, consisted of the elements which were put into the furnace to raise the heat, and of those which were drawn in by the draught—in a word, the elements of combustion—but about the carrying constituent there is a great deal to be said after we know more about it. It seems to us from all this that the hydrogen gas in Sir Robert Ball's tube was not made to glow by heating up to the temperature of incandescence, but somehow by the electricity passing through it, if it did pass. We, therefore, come to the conclusion that the light of nebulæ does not come from gas—or what we call gas—heated up to be incandescent merely to make it glow, and that it might be as cold as the light that comes from the aurora, or as that of a glowworm. Sir Robert Ball refers to stellar points seen through the nebula, and acknowledges that part of the glow may be due to them, which shows that the nebula must have been excessively tenuous ; for we know how thin a cloud will hide Sirius from us, and we think that nobody will assert that two grains of matter dispersed in 1,426,445 cubic feet of space, as we have seen at page 86, would hide Sirius from us. Therefore, we must acknowledge that the glow of nebula in Orion, observed by Sir Robert Ball, was caused either by the stellar points, or by some other thing that most assuredly could not be gas heated to the temperature of incandescence, or in part from both. For we believe that the glowing of nebulæ, fluorescence, phosphorescence, Will-o'-the-wisp, auroras, fire-flies, fire-on-the-wave, etc., etc., all, all proceed from the same cause.

We may now proceed to say a few words about the separation of the rings for the planets, brought about by the rotation of the nebula on its axis, and the centrifugal force produced throughout it thereby. We have shown, at page 88, that a ring could not be detached from the nebula at once in one large annular mass, as it seems to have been the common notion was the mode of separation; and we shall now try to show with some detail what the process must have been, notwithstanding that it has been in a general way described by others; because, like everything else, there is something to be learnt from it. For this purpose we shall select what we have called the Jovian nebula, because we can suppose, for the present, it must have been more nearly in the form of a sphere than either the original or any of the exterior nebulæ, which may not have been properly licked into shape, as it were; and also because we have found that the thickness and mass of the ring for his, Jupiter's, system were vastly greater than those for any other one of the planets. We have made the Jovian nebula to have been 1,370,800,000 miles in diameter, and the greatest thickness of the ring detatched from it to have been 1,406,771 miles. Now in a circle of that diameter, a chord of the length of that thickness would subtend an arc of very little more than 7 minutes, one half of which we shall suppose to be measured on each side of the equatorial diameter of the nebula at right angles to the diameter; then, the middle ordinate of a chord of 1,406,771 miles long, would be 359 miles long. This length would be a very small fraction of the radius of the circle which would be 685,400,000 miles long, but in a rotating sphere of the same dimension, we must acknowledge that the centrifugal force at the middle of the arc would be greater—however small the difference—than at its ends, and would sooner come to balance the force of gravitation ; therefore we must admit that the process of separation would begin there by abandoning a thin layer of matter, convex on the outer side and in a measure concave on the inner side, for the reason just given, much the same as a layer that could be pealed off from the equator of an orange—the poles and equator of an orange are easily distinguished. As the velocity of rotation increased another layer would be abandoned following the first, so far curved on both sides, i.e. convex and concave, and the same process would continue on and on, according as the centrifugal force continued to balance that of gravitation, till the whole of the matter for all the attendants of the sun was abandoned ; so that in the process itself no such division of rings as we have been following could have taken place, but one continuous sheet, as it were, would be formed from first to last. Whether the thickness of the ring for Jupiter's system, or any other system or planet, was limited to the length of the chord we have been dealing with, or came to be many times greater or even less, makes no difference on our explanation. After being abandoned in a sheet, as we have shown it would be, the centrifugal force they had acquired would, for a time at least, keep the particles of the sheet near the radial positions they then occupied, and their mutual attraction would go on diminishing its thickness, till finally the radial attractions among the particles divided the sheet into entirely separate rings after the manner of those of Saturn ; which would in due course break up and form themselves into the smaller nebulæ from which the planets were supposed to have been made.

M. Faye has made it a great point against the nebula hypothesis that when these rings broke up, the rotary motions of the planets resulting from them would be retrograde, because the outer parts of them would be travelling at a slower rate than the inner ones, and has taken the trouble to construct a diagram to show how this would be the case ; but he himself has told us, in "L'Origine du Monde," that Laplace had duly considered this point, and had shown how the friction of the particles of the flat rings among themselves would, through course of time, retard and accelerate each other, so that a ring would come to revolve as if it were one solid piece, and consequently that the outer edge of the ring would come to be travelling faster than the inner one, which according to his (M. Faye's) own showing would produce, on breaking up, a planet with direct motion of rotation. Laplace's words, as cited by him, are :—"Le frottement mutuel des molécules de chaque anneau a dû accélérer les unes et retarder les autres jusqu'à ce qu'elles aient acquis une même mouvement angulaire. Ainsi les vitesses réelles des molécules éloignées du centre de l'astre ont été plus grandes. La cause suivante a dû contribuer encore à cette différence de vitesse : les molécules les plus distantes du soleil et qui, par les effets du refroidissement et de la condensation, s'en sont rapprochées pour former la partie supérieure de l'anneau ont toujours décrit les aires proportionnelles aux temps, puisque la force centrale dont elles étaient animées a été constamment dirigée vers cet astre ; or cette constance des airs exige un accroissement de vitesse à mesure qu'elles s'en sont rapprochées. On voit que la même cause a dû diminuer la vitesse des molécules qui se sont élevées vers l'anneau pour former sa partie inférieure."

In his method of bringing all the molecules of matter in a ring, to revolve round the centre as if they formed one sole piece, Laplace does not appeal to any accommodating force among them except friction, while he might have called in that of the collisions of the molecules amongst themselves. It is not to be supposed that each molecule would remain fixed in the position it occupied when separated from the nebulæ, and only went on rubbing against—and creating friction with—its neighbours, and only creeping closer to the centre or farther from it, as it was acted upon by the attraction of the other parts of the ring. The molecules would be rushing against each other in all directions, in spite of, although in the main obedient to, the law of attraction ; and we could conceive the possibility of molecules gradually working their way from the extreme outer edge to the extreme inner edge of a ring, or *vice versâ*, which would be a much more effectual means of bringing about one period of revolution throughout the whole ring, than the simple force of rubbing against each other. When physicists get a gas shut up in a close vessel, they grant to its molecules the power of committing exactly the same kind of freaks ; and a planetary ring is, to all intents and purposes, a closed vessel to our molecules ; because they have been placed in it by the laws of attraction and centrifugal force, and there is no other force acting upon them sufficiently powerful to liberate them from it. Therefore there is no reason why a molecule in a ring should be always wedged up in one place, especially after we have shown that each molecule of matter, in any of the rings we have been dealing with, must have had a much greater free path to move about in, than a molecule of gas shut up in any of the vessels used by physicists.

We have no reason to look upon the rings of Saturn otherwise than as in process of being converted into one or more satellites, most probably more than one ; because if the matter they are composed of has been separated from the planet in the form of a sheet, the same as we have seen must have been the case with the matter separated from the original nebula for the planets, the sheet has been already divided into at least three distinct parts, and surely that cannot have been done without some object. If these rings have been left, as has been said, in order to show us how the solar system has been formed, that does not authorise us to conclude that they will always remain in the form they have. There is no reason why the lesson should not be carried out to the very end, through the breaking up of the rings, formation of spherical nebiculæ, and finally satellites. It would be rash to assert that the matter of which any one of them is composed—be it atoms, molecules, meteorites, or brick bats—cannot, through friction and collisions of its particles among themselves, come to revolve around Saturn as if it were one solid piece. But should anyone do so, and adopt M. Faye's condemnation of Laplace's mode of forming rings, he must confess that when Saturn's rings are converted into satellites, their rotations must be retrograde ; and it might be, for him, an interesting inquiry to find out whether the rotations of the existing satellites are direct or retrograde.

Astromoners have learnt the lesson as far as it has gone, have noted and registered the state of affairs as it is at present, and their successors will no doubt do the same as changes succeed each other. The day may be inconceivably remote, but it will inevitably come for the rings to be changed into satellites, unless they are disposed of in some other way. It has been said that were the rings to break up, in consequence of their being in a state of unstable equilibrium, they would fall back upon the planet, but that would depend on circumstances. If the motion of their revolution were stopped altogether, they would certainly fall back upon the planet ; but if it were not stopped then each molecule would retain its centrifugal force, and would revolve around the primary on its own account, just as, according to very general opinion, it does at present. We do not see why, or for what purpose, these rings could have been separated from Saturn merely to fall back upon him again. It would be rather a strange way of giving a lesson if it were stopped, by a cataclysm of some kind, just when the most interesting part of it was in a fair way of being exhibited. Such a proceeding would assuredly not suit the ideas of those who believe that the solar system has been self-formed by a simple process of evolution.

During the whole process of separation of rings from the original nebula, the nebulous matter would be abandoned in what we may call the form of thin hoop-shaped rings, so that the equatorial region of the nebula would be flat—as we have shown at p. 115—and when the nebula came to be so much reduced that it could abandon no more matter through centrifugal force, its form would be, in some measure, like that of a rotating cylinder terminating at each end in a cap in the form of a segment of a sphere. When explaining the formation of planetary rings, we have seen that in the Jovian nebula the length of the flat part would have come very soon to be nearly 1,500,000 miles, and that it would increase rapidly. But, remembering that the flattening of the equatorial part must have begun on the original nebula, we see that the flat part must have increased vastly in length before it reached Jupiter, and that by the time the residuary, or solar, nebula was reached—which we made to be only a little over 63,000,000 miles in diameter—the cylindrical part of it would bear no small proportion to that diameter. Taking this form of the nebula into consideration, and also the fact that the separation of matter from it by centrifugal force could not always be absolutely equal all around it, the swaying in its rotary motion produced by the all but inevitable inequality of mass, at the two ends of the cylindrical part, and at the sides of the segmental caps, may have been the cause of the differences in the inclinations of the orbits of the planets to the ecliptic ; and especially of why the difference came to be so much greater in the case of Mercury than in any of the others.

In connection with this very reasonable conclusion as to the form of the nebula almost from the beginning, we may add that, when it ceased to throw off rings, it would be very much in the same condition as Saturn is at the present day. Therefore we may conclude with very great safety, that the present form of Saturn is that of a cylinder with segments of spheres forming the ends ; and in this manner can account for his square-shouldered appearance, which has puzzled more than one astronomer.

The idea has been very general that in condensing and contracting, the nebula would gradually come to assume the form of a lens of a very pronounced character, from the circumference of which the rings would be abandoned one after the other ; but when thoroughly looked into, it is difficult to see how this could be the case. In a sphere of cosmic matter contracting equally all round towards the centre through the force of attraction, it is more natural to suppose that the separation of matter from its equator through centrifugal force, would have a tendency to diminish the equatorial more rapidly than the polar diameter, as we have been trying to show above, more especially as the attraction of the matter in the rings as they were abandoned one after the other would, in a constantly increasing degree, assist the centrifugal force in facilitating the separation by drawing the matter outwards. Matter falling in from the polar regions would afterwards require to have its motion turned off at right angles before it could be sent off by centrifugal force to the equator, an operation which would be more easily effected in the equatorial regions, where the gravitating motion had only to be retarded; and as very unequal amounts of density could not be created in the interior parts of such a sphere by gravitation, so as to cause pressure outwards, it is difficult to show how the polar diameter could be more rapidly reduced than the equatorial diameter, which was being continually shorn of its length. It may be said that all that we have been writing in the last few pages is absurd, because we have been proceeding on the supposition that the condensation of the nebula was effected at or near its surface. Laplace procured this condition by piling up imponderable heat in his nebula, but he might have got it otherwise. Given a nebula such as the one we are dealing with of 6,600,000,000 miles in diameter, where would condensation be most active? Most undoubtedly where there was the greatest mass of matter. Compare, then, the mass of 1,000,000 miles in diameter at the surface with the mass of the same diameter at the centre, and we cannot hesitate for a moment in concluding that the most active condensation would not be very far from the surface. Not only so, but the same would continue to be the case, at least until the last ring was abandoned. Thus by working upon what may have appeared to be an absurd foundation, i.e. condensation at the surface due to the intense heat of the nebula, we have been able to acquire more correct ideas than we had before, of how the solar system was elaborated. But we shall have much more to say on the same subject hereafter.

There has been a great outcry raised about the rotation of the planets Neptune and Uranus being retrograde, as is correctly concluded to be the case from the revolution of their satellites being retrograde, but we do not see that there has been any good reason for it. Laplace, no doubt, concluded, wrongly, that the motions of all the bodies of the solar system—as known to him—were direct, and therefore used that conclusion in showing that there were 4000 milliards against 1 in favour of his hypothesis being right ; but at the same time it cannot be concluded that he thought that it would be destroyed by the motion of rotation of one or even several of the forty-three bodies turning out to be retrograde ; because, when discussing the hypothesis of Buffon, he states, most distinctly, that it is not necessary that the rotation of a planet should be in the same sense as that of its revolution, and that the earth might revolve from east to west, and at the same time the absolute movement of each of its molecules might be directed from west to east. His words as cited by M. Faye in "L'Origine du Monde," at page 158, are : "A la verité, le mouvement absolu des molécules d'une planète doit être alors dirigé dans le sens du mouvement de son centre de gravité, mais il ne s'ensuit point que le mouvement de rotation de la planète soit dirigé dans le même sens ; ainsi la Terre pouvait tourner d'orient en occident, et cependant le mouvement absolu de chacune de ses molécules serait dirigé d'occident en orient, ce qui doit s'appliquer au mouvement de révolution des satellites, dont la direction, dans l'hypothèse dont il s'agit, n'est pas nécessairement la même que celle de la projection des planètes." He seems to say, "This would suit Buffon's hypothesis, but I do not require it for mine." Even were this not so, it would not be very difficult to account for the retrograde rotation of these two planets, but we are not yet prepared to show, in a convincing manner, how these motions were produced. We have to show first how the nebula itself was brought to the dimensions at which we took it up, and there is a great deal to be done before we can show that.

Should our belief in being able to explain how the retrograde rotations of Uranus and Neptune were brought about turn out to be unacceptable, we would not condemn the nebular hypothesis, because, as M. Faye himself says, if we add the asteroids to Laplace's 43 we should have somewhere about 500 bodies, all with direct motion, agreeing with the hypothesis, against 4 that do not, that is about 125 to 1 instead of 43 to 1, which was all Laplace could claim. Moreover, we have not been able to see that M. Faye's objections to it are well founded, rather the contrary ; nor can we agree with him when he says that when one point in a hypothesis is found to be erroneous it ought to be abandoned altogether, and something better sought for. Is his something any better? All acquired knowledge has been built up from ideas collected from all sides, and from errors reformed. What would a grammarian say were we to return to him his grammar as useless, because we had found one exception to one of his rules against 125 cases in which we had found it to be right? Perhaps it would put him in mind of the name of a tree. And grammar is not the only case in which we say that the exception confirms the rule.

In taking the nebula to pieces, we have taken no notice of the satellites of Mars, not only because they are so small that they would have had no sensible effect on our calculations, but because we cannot conceive that they could have been abandoned by the planet, when in a nebulous state, in the same manner as the planetary rings are supposed to have been by the parent nebula ; and we might simply refer to the dimensions, especially the thinness, we have found for the ring out of which Mercury was formed, for proof of our assertion ; but for more satisfactory corroboration, we will go a little deeper into the affair. Let us take the diameters of Mars and of the orbits of his satellites, as they are stated in text-books of astronomy ; that is 2957, 11,640 and 29,200 miles respectively, and suppose the diameters of what—in the method we have applied to the planets—we would call the Deimos and Phobos nebulæ to have been 40,000 and 20,420 miles also, respectively ; then these two diameters would make the breadth of the ring for the formation of Deimos to have been 9790 miles. With these data, if we go through a series of calculations with respect to this outer satellite, in all respects similar to those we have made for each of the rings of the planets, we shall find that the ring for Deimos would have been only 5·64 *inches* thick, without taking into account its condensation during the process of separation. This, of course, points out at once the impossibility of any such operation going on in Nature. We can imagine the possibility of a ring of even millions of miles broad, and of very great tenuity, holding together provided it be hundreds of thousands of *miles* thick, but to think of one 10,000 miles broad and less than 6 *inches* thick holding together is another affair altogether. With respect to Phobos, it is only necessary to say that he revolves round Mars in considerably less than one-third of the time that he ought to, and is therefore not a legitimate production of the nebular hypothesis any more than Deimos can be. Here, then, we have come upon two bodies, one of which has not been formed in the way, and the other has not the proper motion, prescribed in the hypothesis; but we do not think ourselves justified in declaring it to be worthy of condemnation on that account, seeing that we have found no other difficulty in working out the solar system from it.

Moreover, it is not impossible, nor do we think it at all improbable, that through the course of time astronomers may discover that Phobos is a captured asteroid—perhaps Deimos also—gradually working its way into final annexation. And who can tell how many of these erratic bodies Jupiter and Mars may have captured already? In the dark as it were, for they may have been too small to be noticed when they were being run in. Neither of these two worthies has ever been very much celebrated in song or history for respect for his neighbour's property. Jupiter is credited with sorting out the asteroids and arranging them in bands, and perhaps he has been human enough to exact a commission for his labour; and it might be more in his line, and certainly much more easy for Mars, to take forcible possession of as many of them as came within his reach.

CHAPTER VII.

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COMING back to the period when we reduced the residuary nebula to the density of our atmosphere with temperature of 0°, or freezing water, we can with confidence affirm that none of the rings abandoned by it for the formation of planets, could have carried with them any contingent of heat to help them in their formation—any beyond the temperature of space—for even if they did it would very soon be reduced to that. Each one of them in condensing, breaking up, rejoining the broken fragments, converting itself into a minor nebula, and finally constituting itself as a planet, must have accumulated in the process its own heat requisite to convert it into a molten liquid globe—a stage of existence through which they are all, that is, the major planets, acknowledged to have passed, or have to pass. During that process its primitive annular form, and the multitude of fragments into which each one of them broke up, would present sufficient radiating surface, not only to dispose of all the heat it could have brought with it from the nebula, but a considerable part of the little it could create for itself while contracting and condensing. We may even go farther and assert that no one of them would have any necessity for being supplied with extraneous heat until it had, in a great measure, exhausted the stock it had produced for itself, or so far as to cool down from the molten liquid to the solid state, and to the stage when vegetable and animal life could exist upon its surface. We have no reason for supposing that an enormous supply of extraneous heat was crammed into each nebula, merely to be radiated into space before condensation could take place, and thus retard the execution of the work in hand. If there are astronomers or physicists who believe that the sun could not acquire by gravitation, all the heat he must have expended during geological time, they must look for it in some other source than that of useless and impossible cramming.

Hitherto we have said nothing of heat being radiated into space by the nebula during our operations, because there could be almost absolutely none to radiate from it at 0° of temperature. No doubt there is a large range between this and the absolute zero of temperature which is -274°; but we have seen, at page 99, that when the nebula was condensed from 403,000,000 to 274 times less dense than air, only *one degree* was added to its temperature—that is, it was raised from -274° to -273°—and that these -273° of absolute temperature were added to it in its condensation from being only 274 times less dense than air to atmospheric pressure, when its temperature became 0° of the ordinary Centigrade scale. Therefore the only period when there could be any measurable radiation of heat into space would be between the times when the diameter of the nebula was (see Table III.) between 58,000,000 miles and 9,000,000 miles. Even when the end of this period came, the temperature, after a contraction of 49,000,000 miles in diameter, would be only $-1°$ raised to $0°$—in other words $-273°$ raised to $0°$—and that would not furnish much positive heat—heat such as we are accustomed to deal with—to be radiated into space, whose temperature is without doubt somewhat warmer, so to speak, than $-273°$. And let us repeat, and fix it in our memory, that this $-273°$ was equal to only $1°$ of positive heat.

If we now suppose the nebula to be condensed to one-tenth of its volume, with consequent density of 10 atmospheres, and corresponding diameter of about 4,150,000 miles, its temperature would be 2740° of the ordinary Centigrade scale—according to our mode of calculating hitherto—provided no heat had been radiated from it into space in the meantime. Of course this could not be the case, but we have no means of calculating what the amount of radiation would be, and it will not make much difference on our operations to take no notice of it. However, it is here necessary to take into consideration that 2740° would be the average temperature of the nebula; consequently, if condensation was most active where the greatest mass was, which certainly could not be at the centre or even near it, there also heat would be produced most rapidly, from whence it would spread towards the centre and surface. From the centre it would have no outlet, and would accumulate there as condensation advanced; whereas from the surface it would be radiated into space, and would tend to decrease in amount, so that we may conclude that the surface must have been considerably colder than the centre. If to this we add the fact that, in order to get to the surface, heat would have to be conducted, or conveyed by currents, over from one to two millions of miles, it becomes all the more certain that the central heat would be very much greater than that of the surface. How much less it would be at the surface we cannot pretend to calculate, but we may suppose it to have been from one-fifth to one-third of the average, or rather, somewhere between 370° and 1000°, which we have taken, at page 110, to be the temperatures of red-heat and white-heat. And thus we come to find that the nebula, which was supposed to be endowed with excessive heat when it extended far beyond the orbit of Neptune, could not have radiated either heat or light into space to much purpose, until it had been condensed into not much more than 4,000,000 miles in diameter. This then we must acknowledge to be the earliest period at which the sun began to act as the life sustainer of his system; because, even were it to be found that there are other planets revolving within the orbit of Mercury, which we do not think very probable, we have seen that he could have no light or heat with sufficient vivifying power to radiate to them, till his diameter was reduced to not far from what we have shown above. Even then the sun would most likely be very much less brilliant than he is now, but the light may have been sufficient to promote vegetation on Mars—or the earth, if it was sufficiently cooled down from its molten state—and not much heat would be required by him, as there would probably be a remnant of his own interior heat, still sensible at the surface, sufficient for vegetation at least.

We have had occasion to refer several times to the temperature of space, and, though we cannot pretend to determine what it is, our operations enable us to show that it must be very much less than any estimate of it that has ever come under our notice. The nearest approach made to absolute zero by M. Olzewski, in his experiments on the liquefaction of gases, as reported in the "Scientific American" of June 2, 1887, was −225°, or so-called 49° of absolute temperature,* which would correspond to a density of 0·1788 of an atmosphere. This could not be the density of space, because it can be easily shown that our nebula, when at the same density, must have had a diameter of about 29,000,000 miles, and we must admit that were a globe of this diameter rotating in a medium of its own density, the friction between the two

* From the same source, date June 6, 1896, we learn that the greatest cold probably ever reached was −243·5° or 31·5° of so-called absolute temperature, but that will have very little effect on our calculations, and so it is not worth while altering them all to suit.

would have been so great as to put a stop to the rotation before very long. We may even say that distinct rotation could never have been imparted to it. Following the same reasoning, we must acknowledge that the density of space must be much lower than that of our original nebula, if that could be, and therefore we can assert with confidence that the temperature of space must be far below $-225°$.

Here our operations put us in mind that we have said nothing yet about the ether, or what effect it might have on our nebula and the bodies formed out of it. We have not done so for the simple reason that, with one exception, it has never been taken into account in any scientific work that has come into our hands, except so far as its being called upon to perform the offices of a dog that has been taught to carry and fetch, and we have not known how to deal with it. But as we have come along, we have seen that it must have had something to do with the density, and consequent temperature, of all the bodies we have been dealing with, and that, if properly studied, it may enable us to account for some things that we have never seen, to our mind, properly explained. We know that it was devised, or conceived of—somewhere between thousands of years ago and the birth of modern astronomy—as a medium for carrying light, heat, and anything that was hard to move, through space, or to where it was wanted to be moved, by its vibrations or undulations, in the same way that sound is conveyed by wave motion, or vibration, through air, water, and a multitude of bodies; and we understand that some time during that long period it began to be looked upon as a material substance. We are told that it is supposed to pervade all bodies of all classes, but we think this idea must be taken in a limited sense, because, whether it is combined with electricity, as some suppose, or is only a carrier of electricity, a good conductor must have a larger supply of it than a bad one, and an absolute non-conductor, if there be such a substance, must contain none at all, always provided the ether is the conducting or carrying power. We are told also, that it is neither of the nature of a gas nor a liquid, but may be of the nature of a jelly, and of its nature we shall have more to

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say hereafter. It was natural that it should be conceived to be a material substance, because if light and heat were to be carried from one place to another by wave motion, as sound is by water and air, then the medium for carrying it must be of the same nature as air and water—or any other carrier of sound—that is, it must be a material substance and, in consequence, possessed of some density or specific gravity. The only place where we have seen any density assigned to it has been, in a series of articles on the "Origin of Motion," published in "Engineering" of 1876, where it is estimated to be $\frac{1}{5.264.800}$th* of the density of air. How this estimate was formed is explained in the number for December 1, 1876, page 461, from which we make the following very long quotation, because we look upon it as of great importance.

"Steel of the best quality in the form of fine wire has been known to bear a tensile strain represented by not less than 150 tons per square inch before breaking, and even this cannot be said to be the limit to the tensile strength of steel, since the tenacity increases as the diameter of the wire is reduced. Rejecting 'action at a distance,' therefore, these molecules of the wire must be controlled by some external agent, and therefore, the pressure of the external agent must *at least* equal the static value of the strain. The pressure of the ether therefore cannot be less than 150 tons per square inch. Now, since it is a known fact that the strain required to separate molecules in 'chemical union' would be very much greater than in a mere case of 'cohesion,' it follows that the ether pressure must be greater than the above figure. If we suppose the strain required to separate the molecules of oxygen and hydrogen combined in the state of water (one of the most powerful cases of chemical union) to be only three times greater than in the case of the molecules of steel, then this would give 450 tons per square inch as the effective ether pressure. It may be taken as certain that the strain required

* Years after this was written we have seen it stated that the density of the ether has been calculated from the energy with which light from the sun strikes the earth, and that to represent it there are twenty-seven cyphers after the decimal point before the figures begin. But as this gives something like one thousand quadrillionth part of the density of water, we refuse to accept or even think of it.

would be greater than this, as it has not been found possible by any ordinary mechanical means to separate molecules in chemical union. However, as it is only our object to fix a limiting value for the ether pressure, or a value that is less than the actual fact, we will therefore take in round numbers 500 tons per square inch as the total ether pressure, having thus valid grounds for inferring that this estimate is within the facts as they actually exist. The existence of such a pressure as this might well be sufficient to strike one with astonishment and legitimately excite incredulity, if it were not kept in mind that this pressure is exercised against *molecules* of matter, a perfect equilibrium of pressure existing, so that it may be deduced with certainty beforehand, that, however great this pressure might be, it could not make itself apparent to the senses. The air exercises a pressure of some tons on the human body without such pressure being detected, how much more cause, therefore, is there for the perfect concealment of the ether pressure, which is exercised against the molecules of matter themselves. This great pressure is the absolutely essential mechanical condition to enable the ether to control forcibly the molecules of matter in stable equilibrium, and to produce forcible molecular movements when the equilibrium of pressure is disturbed (as exemplified in the molecular movements of 'chemical action,' etc.).

"It is generally admitted that the ether must have a very low density, one reason being the almost imperceptible resistance opposed by it to the passage of cosmical bodies (the planets, etc.) at high speed through its substance. The pressure of an aëriform body constituted according to the theory of Joule and Clausius, being less as its density is less, it will therefore be necessary to show that the ether can exert so great a pressure as the above, consistent with a very low density. From the known principles belonging to gases, the pressure exerted by an aëriform medium is as the *square* of the velocity of its component particles, and as the density. We will, in the first place, consider what the density of the ether would be, if it only gave a pressure equal to that of the atmosphere (15 lb. per square inch). From the above principles, therefore, it follows that for the ether to give a pressure equal to that of the atmosphere, the ether density will require to be as much less than that of the atmosphere, as the *square* of the velocity of the other particles is greater than the square of the velocity of the air molecules. The velocity of the air molecules giving a measure of 15 lb. per square inch is known to amount to 1600 feet per second. Taking, therefore, the square of the velocity of the ether particles in feet per second, and the square of the velocity of the air molecules and dividing the one by the other, we have the number of times the ether density must be less than that of the atmosphere, in order for the ether to give a pressure of 15 lb. per square inch, or we have

$$\frac{(190{,}000 \times 5280)^2}{1600} = 393{,}120{,}000{,}000.$$

This result shows therefore that the density of ether, if it only gave a pressure equal to that of the atmosphere, would be upwards of 390,000,000,000 times less than the density of the atmosphere. This result expresses such an infinitesimal amount of almost vanishing quantity, that the ether density might be well much greater than this. We will now, therefore, consider what the ether density would be to give a pressure of 500 tons per square inch. Pressure and density being proportional to each other, it follows that for the ether to give a pressure of 500 tons per square inch, the ether density would require to be as much greater than the above value, as 500 tons is greater than 15 lb. Multiplying, therefore, the above value for the density by this ratio, we have

$$\frac{1}{393{,}120{,}000{,}000} \times \frac{500 \times 2240}{15} = \frac{1}{5{,}264{,}800};$$

or this shows that the density of the ether to give a pressure of 500 tons per square inch would be only $\frac{1}{5{,}000{,}000}$th of the density of the atmosphere. This value representing a density less than that of the best gaseous *vacua* is therefore quite consistent with the known fact of the extremely low density of the ether. It follows, therefore, as a mathematical certainty dependent on the recognised principles belonging to gaseous bodies, that the ether could exert a pressure of not less than 500 tons per square inch consistent with such an extremely low density as to harmonize with observation."

If the ether is possessed of a density equal to that shown above, then the density of our original nebula must have been greater than what we have shown it to be. The density we found for it was $\frac{1}{403\,000,000}$th that of air, or 0·000000002481 of an atmosphere, and $\frac{1}{5,264,800}$th is equal to 0·00000019 of an atmosphere; if then we add these two together we get 0·0000001925 of an atmosphere as the density of our nebula. This comes to be very slightly greater than the density of the ether, and shows that the estimate in the foregoing quotation is too high; unless it is asserted that the ether can exert no frictional action at all, which, we believe, no one has ever done; while the absolute temperature of the nebula at the new density would be 0·000053°, which would be a very small addition indeed to the 0·00000068°, we found for it at first. On the other hand, when the nebula was reduced to 29,000,000 miles in diameter the density of the ether would have increased its density from 0·1788, which we showed it then to have, only to 0·17880019 of an atmosphere, which would make no appreciable difference on its temperature, and would be so immensely greater than the 0·00000019 of an atmosphere of the ether that it could hardly be supposed to have any effect in retarding the rotation of so much heavier a body. And should it be found that the density of the ether is $\frac{1}{4}$, $\frac{1}{3}$, or $\frac{1}{2}$ less, or even a great deal more, than that shown in the above quotation, it would only have proportionately less effect on our nebula, in every sense, than what we have just shown. We may, therefore, conclude that the introduction of the element ether has not vitiated our operations in any way up till now, and we shall leave it until we have acquired more knowledge of its nature and effects.

Although we have already condensed our nebula to somewhere about 4,000,000 miles in diameter, where we have shown it might begin to radiate light—radiation of heat may have begun when the diameter was ten times as great, or even before that—we propose to return to the period when it had just abandoned the ring for the formation of Mercury and was 63,232,000 miles in diameter, and became what we have called the Solar nebula; because there is a good deal to be learned from a careful study of our operations up to that period, and of what must have taken place during further condensation up till the final establishment of the sun such as it is at the present time.

When the planet Neptune was discovered, Bode's Law fell into disrepute for a time, because the new planet was found to be much nearer to the sun than, according to it, it should have been. All the other planets occupied the places assigned to them within 5 per cent. of the exact appointed distance from the sun, but Neptune turned out to be 22·54 per cent. out of his exact place, and hence the discredit thrown upon the law. It was hard treatment for a servant that had helped so unmistakably—as we know to have been the case—to the discovery of the first four asteroids, which has afterwards been followed by the discovery of a whole host of them, and that had been pressed into the service for the discovery of the very planet which was the cause of its discredit—but such is the world. However, first offences against the law are generally looked upon with merciful eyes, and the Series of Titius seems to have been so far received into favour again that, some astronomers are said to have been looking out for another planet farther off than Neptune, being convinced that there must be some reason why a law that has shown itself to be right in eight cases should be altogether wrong in the ninth. Here, we think that the most likely explanation that can be given is, that the ring out of which Neptune was formed divided itself, after breaking up, into two planets instead of one, and that this is the reason why, Bode's Law could not point out the true position of either of them. It is hard enough to believe that the ring out of which Uranus was made—which we have seen may have been 954,000,000 miles broad, and over 3,400,000,000 miles in extreme diameter—could have united its fragments, after breaking up, into one planet, and the difficulty of belief becomes greater the greater the diameter comes to be. We have, in our work, considered the breadth of Neptune's ring to have been 1,010,000,000 miles, but then we limited the diameter of the nebula to 6,600,000,000 miles —we had to draw the line somewhere—whereas it may have been a thousand million miles greater, which would very greatly increase the probability of two planets, perhaps even more, having been formed out of the ring. If it has been so, the law could not apply to the case. A new Act was required. Besides, it is not a law, never has been, but only a register of facts; and we know that truths are often discovered from similar registers. It registers, and at the same time shows, that there is a nearly fixed inter-relation, even proportion, in the distances of the planets from the centre of the sun as far out as Uranus; and were we to make a similar register, beginning at the (present) outside of the planetary system, and registering the number of revolutions, beginning with 1 for Neptune, rates of acceleration of revolution in number of days, and densities of the planets, we may draw from it some useful knowledge. But we shall first extend Bode's Law to embrace Neptune, and show the discrepancies between the actual positions of the planets and those pointed out by the law.

Here we see that, with the exception of the first step from Neptune to Uranus which is only 1·9577, we have an average gradation of acceleration of 2·5898 times, from one planet to another, from the outermost as far in as Mars; and that had Neptune had the period of revolution sought for by Leverrier in his discovery of that planet, viz. 217·387 years, or 79,399·602 days, the average rate of acceleration would have been 2·5895 times, from planet to planet, as far in as Mars. This, we think, is pretty strong evidence that one law of acceleration was in force from the beginning of the separation of rings from the nebula up to the time when the ring for Mars was separated—the departure from it in the case of Neptune, notwithstanding—and goes far to prove that part of the nebular hypothesis which implies that each of the planets is now revolving round the sun in the orbit, and with the velocity, belonging to the centre of gyration of the ring out of which it was formed. From Mars to Venus the law—the areolar law, of course—had changed to a variable decreasing

BODE'S LAW EXTENDED.

—	Mercury.	Venus.	Earth.	Mars.	Asteroids.	Jupiter.	Saturn.	Uranus.	Neptune.
Numbers in progression	0	3	6	12	24	48	96	192	384
Add 4 to each for distance from sun, Earth's being 10 . .	4	7	10	16	28	52	100	196	388
Distance from sun according to the law, in miles	37,186,000	65,075,500	92,965,000	148,744,000	260,300,000	483,418,000	929,650,000	1,782,114,000	3,607,042,000
Actual distance	35,987,000	67,245,000	92,965,000	141,650,000	..	483,678,000	886,779,000	1,783,383,000	2,794,000,000
Percentage of distance beyond law .	..	3·34	..	..	..	0·05	..	0·07	..
Percentage of distance within law	3·22	..	..	4·77	..	..	4·50	..	22·54

Our register as specified above will be the following :—

—	Neptune.	Uranus.	Saturn.	Jupiter.	Asteroids.	Mars.	Earth.	Venus.	Mercury.
Revolution of Planet, in solar days .	60,180·86	30,688·30	10,759·2198	4,332·5848	1,714·1876	686·9796	365·2563	224·7007	87·9692
Acceleration of revolution, Neptune taken as 1	1	1·9577	2·8523	2·4833	2·5606	2·4629	1·8808	1·6255	2·5543
Densities of Planets . . .	1·132	1·302	0·736	1·358	..	4·188	5·660	4·810	6·850

law, as seen from the foregoing register, which then again changed into an increasing one, till at Mercury the rate of acceleration rose again to 2·5543 times from Venus, or very nearly the same rate of increase that existed from Uranus to Mars. The causes of these changes may or may not be able to be accounted for—we shall have to return to them hereafter, in the cases of Neptune, the earth and Venus—but there is one thing of some importance that is deducible from the register, which we shall endeavour to make clear.

A good deal has been written about planets or other bodies existing between Mercury and the sun, especially about Vulcan whose existence seemed to be so certain, that his distance from the sun and period of revolution were calculated to be about 13,000,000 miles and 20 days respectively. Now, with what we have seen about the rate of acceleration of planets as their orbits approach the sun, we may endeavour to form some notion of where any within the orbit of Mercury may be found. If we take the same rate of acceleration we have found between Venus and Mercury—that is 2·5543, which may be lookod upon as almost the general rate for all the planets—we find that there might be a planet revolving round the sun in 34·4436 days; but here we must stop, because, though we could make no objection to the existence of a planet with the period of revolution just shown, were we to take another equal step towards the centre of the nebula, the same acceleration of rotation would give us a planet, or ring for a planet, revolving round the sun in 13·4454 days; not much more than one-half the average of his rotation round his axis at the present day, which would knock on the head most completely the theory that each planet was detached from the nebula at the time that it was rotating with the velocity of the planet's orbit, or we should have to conclude that the nebula had passed, by a long way, its power to abandon matter through centrifugal force. No one could suppose that a ring for a planet could be formed within the body of the nebula and abandoned, or thrown out, afterwards, because centrifugal force could not throw out the ring and at the same time retain the surrounding matter.

Turning our thoughts now to the supposed planet Vulcan, which was calculated to revolve round the sun in about 20 days, we have either to conclude that it was formed in the body of the nebula and come to the same breakdown of the nebular hypothesis, or we have to acknowledge that the sun is now rotating much more slowly on its axis than the nebula did at the time the ring for Vulcan was abandoned.

If we now direct our attention to the densities of the several planets, we shall find some suggestive matter in their study. A general look shows us at once that there are four periods of rise and fall in their densities. There is one rise and fall (referring to our register) from Neptune to Uranus and on to Saturn ; then another rise to Jupiter and fall to, we suppose, the asteroids, because we are told that the quantity of matter in the region where the asteroids travel is less than in any other zone of the solar system, and the general density must in consequence have been less there than anywhere else ; still another rise from the Asteroids to the Earth, and fall to Venus ; and then a final rise to Mercury accompanied, without doubt, by a fall after the planet was abandoned, because the centrifugal force of the rotating nebula must have been decreasing, at the least, preparatory to its ceasing to have the power to throw off more matter. The first rise and fall would seem to indicate that there had been a much closer mutual relation in the births of Neptune, Uranus and Saturn than is indicated in any way in the nebular hypothesis. We could imagine that at one time they formed one flat ring, which afterwards divided itself into three, following the same law as we see dividing the rings of Saturn at the present day. With respect to Jupiter, his enormous size is sufficient to entitle us to believe that his ring was separated from the nebula independently of any of the others, and to account for there having been the rise and fall in the density that we have noted between Saturn and the Asteroids. Then the rise and fall from Mars to Venus, or further on towards Mercury as it would be, may indicate one ring divided into three in the same manner as we have supposed for the three outer planets. And the final rise to Mercury and subsequent fall to the sun or to the solar nebula might be either due to one operation or to complication with other unknown bodies that may be travelling between Mercury and the sun.

In support of the foregoing ideas, we may also refer to our having said on a previous occasion, that the whole of the matter separated from the nebula in the form of thin hoop-shaped rings, would condense into one continuous sheet, perhaps even up to the time when centrifugal force could not throw off any more matter against the force of gravitation. In that case we can conceive that the radial attraction, outwards and inwards, of the particles of the matter forming the sheet would gradually establish lines of separation, dividing off the matter into distinctly separate rings, preparatory to their transformation into planets ; but we cannot explain how these separate rings came to be more dense in one place than another. We must leave that for future discovery. Meanwhile the idea of one continuous sheet of matter extending from the sun out to Neptune, suggests the possibility of all the rings having been in existence as rings, more or less advanced in their evolution, at the same time ; and if not so much as that, makes it more easy for us to see how the four inner planets, being made out of more condensed cosmic matter, and being of so much smaller volume, have arrived at a much more advanced stage of their being than the four outer ones. Going a little further, we can see how the cosmic matter of the rings condensing from both sides in the direction of their thickness, and falling in impeded, so to speak, the tendency to contract in length, or circularly, until they arrived at a certain stage of density, when they began to contract in their orbital direction, to break up into pieces, each one of which would form itself into a small, probably shapeless, nebula with a tendency to direct rotation, as explained and shown by M. Faye in "L'Origine du Monde," chapter xiii., page 267, entitled "Formation de l'Universe et du Monde Solaire"—an explanation which must have occurred to everyone who has taken the trouble to think seriously, of how nebulous spheres could be formed out of a flat nebulous ring endowed with a motion of revolution.

We have seen at page 127 that when the nebula was condensed to a little over 4,000,000 miles in diameter, its average temperature might have been 2740°, provided no heat had been radiated into space. In like manner, we can see that the sun being now condensed to 1·413 times the density of water, or 1·093 times the density of air, in other words, that number of atmospheres, its present average temperature might be about 300,000°—as each atmosphere corresponds to 274°—provided no radiation of heat into space had been going on. But this way of estimating could not in any way apply to the nebula after it had ceased to throw off planetary matter; because from that time, or at all events from the time when it came to be of a density equal to one atmosphere and temperature of 0°, or freezing point of water, that would be accumulated within it, owing to the difficulty of carrying to the surface, to be radiated into space, what was produced by condensation in the interior, as we have shown before. Both heat and pressure would increase from the surface towards the centre, the former rising, in spite of surface radiation, to something far beyond what we have stated above that it might be, aided by the increase of pressure which near the centre must be enormously greater than the average of 1093 atmospheres, seeing that the pressure at the surface of the sun is estimated to be not far from 28 atmospheres. The first cause of the increase of pressure would be the condensation produced by gravitation, which according to the areolar law would increase the rotary velocity of the nebula in proportion as the centre was approached; and as this would begin long before it had given up abandoning rings, or rather from the very beginning of its rotation; from that time, there would be different rates of rotation at different distances between the surface and the centre, which would cause friction among the particles of its matter, in other words a churning of the matter shut up in the interior of the nebula, and thus produce heat over and above that produced by the condensation of gravitation alone. If two particles of matter would produce a given quantity of heat, in falling from the surface of the nebula to any point nearer to the centre, they would surely produce more if they were rubbed against each other by churning action during their fall.

Reflecting on what we have written up till now, we see that the analysis of the nebular hypothesis we have made, which at first may have appeared to be unnecessary or even useless, has shown us and made us think over many details, of which we had only a vague notion previously. It has shown us that without condensation at or near the surface of the nebula—which we have pointed out must have been caused by its greatest mass being near that region, and which Laplace procured by endowing it with excessive heat—the various members of the solar system could not have been evolved from it in terms of the hypothesis. From it we have been able to learn, by means of the register of the acceleration of revolution from one planet to another, when, and for what reason, the nebula ceased to be able to throw off any planet nearer to the sun than the supposed Vulcan, or almost even so near. Finally, and not to go into greater detail, it has so far given us some ideas, that we had not before, of the internal structure of the sun, and has made us believe that a great deal may be learnt by attempting to find out what that structure really is. For this purpose, it appears to us that a careful examination into, and study of, the interior of the earth might be a great help, and to this we shall appeal, as we cannot think of any other process by which our object can be attained. This, therefore, we shall endeavour to do in the following chapters.

CHAPTER VIII.

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THE INTERIOR OF THE EARTH AND ITS DENSITY.

BEFORE attempting to inquire into the nature and structure of the interior of the earth, it will be convenient to specify the bases on which the inquiry is to be made, in other words, the data we have to proceed with; which data should be denuded of everything whatever having the semblance of a hypothesis or theory, and should consist of simple facts. Anything founded upon theory must come to an end should the theory be afterwards found to be erroneous, and all the labour would be lost.

What we really know of the earth in this way may be stated as follows:—

Of the exterior or surface we know that it is of a spherical form, surrounded by an atmosphere of probably 200 miles or even more, in height, consisting of common air mixed with vapour of water in more or less degree ; that, of its surface, nearly three-fourths are covered by water, and the remaining fourth consists of dry land, intersected in all directions by rivers ; that on the dry land there are elevated tablelands and ranges of mountains from two to three miles high, with occasional ridges and peaks rising up to altitudes of from five to near six miles, and that in the part covered by water or sea, there are depressions or furrows with depths in them probably exceeding the heights of the highest mountains ; that the sea does not remain constantly at the same level but rises and falls twice in every twenty-four hours, or thereby, in obedience to the attraction of the moon and sun, forming what are called tides ; and that its polar regions are enveloped in dense masses of snow and ice, which the persevering energy of man has not been able to penetrate in centuries of continued and determined effort.

What we know of the interior of the earth is found in great measure from the exterior, that is, from the construction of the rocks as seen in deep ravines, in precipices, and on the sides of hills or mountains ; and also from what we have been able to learn from the exploration of mines and from deep wells, the deepest of which have penetrated it very little beyond one mile in depth ; all of which knowledge may be summarised as follows : That the substances which compose the earth are manifold and of manifold nature—or, more appropriately speaking, simply the elements of chemistry—varying in density, or specific gravity, from the same as that of water, or in some cases much less, to three or four times as much in some kinds of rock and earths (disintegrated rock), to more than twenty times in the heaviest metals ; that from a depth great enough not to be affected by the changes of seasons, the heat of the earth increases in descending towards the centre, by one degree of Fahrenheit's thermometer for every fifty to sixty feet in depth—that is about thirty metres for each degree of the Centigrade scale—as far down as we have been able to penetrate ; that at the greatest of these depths abundant supplies of water are found, which shows that it must exist at much greater depths than any that have yet been reached; and that at unknown depths, as shown by the eruptions of volcanoes, there are masses of matter in a molten liquid state, or that, owing to their great heat, can be suddenly liquefied by diminution of pressure.

Over and above what has been stated, little can be learnt from geology, because the earth must have been formed and fashioned almost to its present condition before geology could begin to exist, and all its teachings are confined to a very few miles from its surface. Its first lesson could only begin when the earth was so far cooled down that a crust could be formed on its surface, and that crust could be deluged by copious falls of rain on it. Some help or guidance may be obtained however, from the ideas which astronomers and physicists have formed on its interior, and it may be useful to have the principal of these ideas specified, as they may help to strengthen arguments that may be advanced, or conclusions that may be drawn.

When it was discovered that the temperature of the earth increases, as we go downwards, at what may be considered a rapid rate, it was calculated that at a depth of from twenty-five to thirty miles, the heat would be great enough to melt any substances that have been found near the surface; and it was immediately concluded that from that depth to the centre the whole of the interior was a molten liquid mass, whose temperature far exceeded any heat that could be produced upon the surface. Even up to the present day, the belief in a liquid interior has not disappeared.

Many years afterwards, the supposed liquid state of the interior of the earth was taken advantage of, to frame a theory that earthquakes and eruptions of volcanoes are caused by the attraction of the moon on the liquid interior producing tides, in the same manner as it produces tides in the sea, which in their turn act upon the crust, cracking and rending it to produce the one, and forcing the liquid matter out through the rents, or up through the vents of volcanoes to produce the other, in some way that it is more easy to imagine than to explain mechanically. Also when the effect of the attraction of the moon on the liquid internal matter came to be duly considered, it was concluded that the crust, with only 25 to 30 miles in thickness, could not be rigid enough to resist the pressure brought upon it by the movements of the interior tides; and it began to be thought that, owing to the pressure of the superincumbent strata, the density of the matter at that depth might be so great that it would become solid at a much higher temperature than it does at the surface; and some physicists went the length of supposing that the earth has a solid crust and solid nucleus with liquid matter between them. On the other hand Sir William Thomson, Lord Kelvin, looking more as it would appear to the effects of the moon's attraction on the crust than on the liquid interior, concluded that the earth must be a solid globe, contracting through gravitation in the interior, and cooling at the surface, because a crust so thin as 25 to 30 miles, or even 100 miles, would be continually rent and broken up by the tidal action of the moon; but Professor Clerk Maxwell and others have thought that the elasticity of the crust would be great enough to admit of its accommodating itself to all the changes of form that would be caused by the action of those tides. Notwithstanding that they agree with Lord Kelvin in the main, in his objections to the existence of a liquid interior, many scientific men suppose that, through the effects of pressure, the liquid interior of the earth may have been changed into a viscous state, as it went on contracting through gravitation, which would, according to the degree of viscosity, either annul, or almost annul, the tidal action on it of the moon. To which it may be added that that action would not raise such high waves in even perfectly liquid molten matter as it would upon water; because it would be easier for the moon to lift a cubic mile of water three or four feet high, than to lift a cubic mile of melted rock or metal to the same height.

Other parties look upon the earth as mainly solid to the centre, but with large reservoirs of liquid matter in various parts of it near the surface, which furnish all the material for volcanic eruptions and are the causes of earthquakes. There are others also who, believing the earth to be altogether solid, consider that when any part of the intensely heated and dense interior is relieved suddenly from pressure, as, for example, by the convulsive action of an earthquake, it will immediately assume the liquid state and become material for volcanic eruptions ; a theory which they consider to be substantiated by the fact of these two phenomena generally accompanying each other. And Mr. Mallet seems to have demonstrated that earthquake-shocks proceed from centres not far from the surface, which would seem to point out that if a liquid interior did exist at 25 to 30 miles from the surface, it could have no part in causing earthquakes. There are others still who consider earthquakes and volcanic eruptions to be caused by water penetrating deeply into the interior, but it is difficult to understand how water could penetrate into the interior to a greater depth than where it would be converted into steam, that is to a greater depth than from three to four miles.

Many other notions about the interior state and conditions of the earth have been formed, more or less entertainable, more or less fanciful, to provide liquid matter for volcanic eruptions. One of these, referred to in "Nature" of December 12, 1889, takes for granted "that granite has consolidated from a state of igneo-aqueous fusion, and that the liquid magma from which all granitic intrusions have proceeded contains water-substance," and proceeds, "It is, therefore, only a further step to assume that this water-substance is an essential constituent of the liquid substratum (assumed by the author), and to suppose that it has been there since the consolidation of the earth." This mixture of water, fire, and molten granite is one that does not agree with what we have been taught of the nature of any of the three components, and we cannot accept it. Why we refer to it more particularly than to the other ideas we have cited, is because it so far comprehends some of them, and that we shall have to return to it hereafter, when we think it will be seen that it has not been properly thought out.

Bearing in mind all these ideas we have cited, and working with the data we have considered as actual facts, we may now proceed with our inquiry.

The belief that the earth is a mass of matter increasing, whether liquid or solid, or part of both, in density from the surface to the centre is so general that we shall look at it in that light first, and endeavour to find out what must be its density at any place between its surface and its centre.

Astronomers and geologists concur in telling us that the mean density of the earth is very near to 5·66 times that of water: knowledge that has been acquired by measuring the attraction of high and precipitous mountains for plummets; by the attraction of masses of metals for each other, measured by the torsion balance; and by the acceleration or retardation of the vibrations of pendulums, as observed in the depths of mines and on the tops of mountains, compared with each other. They also tell us that the average density of the matter and rocks of which the crust is composed is about 2½ times that of water; and then, in a general way, that the average density of the crust, taking into consideration that so much of its surface is covered by the sea, is not much more than 1½ times that of water. This estimate is manifestly incorrect, for it implies that the whole of the crust of twenty-five to thirty miles is affected by the presence of water, when we know that the depth of the sea at any place does not exceed one-fourth of that thickness. Therefore, we shall endeavour to obtain some more accurate computation, as it is the only datum we have to go upon, and has a greater effect upon the result, and upon all things relating to the interior, than might at first sight be supposed.

We find in "Nature," of January 19, 1888, that Mr. John Murray has calculated that if the whole solid land of the earth were reduced to one level under the sea, its surface would be covered by an ocean with a uniform depth of about 2 miles. Here we have a very good beginning for our calculations.

Without taking into consideration the increase of density in water at 2 miles deep, at that depth we may suppose we have come to solid matter, the specific gravity of which could not be less than twice that of water, on account of the pressure of that depth of water upon it. If we now go down

$2\frac{1}{2}$ miles further we shall have the solid matter subjected to a pressure proportioned to that depth ; and if we take its weight per cubic foot at an average between granite (at 163 lb.) and earth (at 77 lb.), or 120 lb., the pressure at $2\frac{1}{2}$ miles deep of solid matter alone will be about 700 tons per square foot, or just about the crushing strain of our strongest granites, and therefore, the density of the matter under it must be equal to that of granite, or 2·5 times that of water. We do not add the pressure of the water, at present, because that may be looked upon by some people as of the same nature as of the atmosphere upon a human body, which neither increases the pressure upon it nor adds to its weight ; but we see that at that depth the solid matter must have a density equal to the average between water on its surface and 2·5—that of granite ; and if we choose to take the average between 2 miles of water and $2\frac{1}{2}$ miles of solid matter, we shall have 1·82 as the average density of the outer $4\frac{1}{2}$ miles in thickness of the crust of the earth. For our purposes, however, and for obvious reasons, we shall consider the average density of the $2\frac{1}{2}$ miles alone of solid matter to be 2·25 times that of water.

We shall now go down to 9 miles deep, because the diameter of 7918 miles we have adopted for the earth will there be reduced to 7900 miles, which will be convenient for our further operations. At that depth we shall have a superincumbent pressure at the very least as follows :—

			Tons.
At 2 miles deep,	2 miles	of sea at 150 tons per mile . .	300
$2\frac{1}{2}$,,	$2\frac{1}{2}$,,	solid matter at spec. grav. 2·25 equal to 331·25 tons per mile .	828
9 ,,	$4\frac{1}{2}$,,	of rock at 163 lb. per cubic foot .	1730
		Total pressure per square foot . .	2858

or just about 4 times the crushing strain of our best granites. Then, as when crushing takes place compression begins, it will, we believe, be far below the mark to estimate the general specific gravity of the earth at 9 miles deep to be 3 times that of water.

We have now added the pressure of the 2 miles of water, because there could be no water at the depth of 9 miles ; for the critical temperature of water is known to be 412°, beyond which temperature water cannot be maintained in its liquid state by any amount of pressure, however great ; and 9 miles would give 483° temperature at 1° for each 30 metres. At that depth there might be steam, although it is difficult to see how it could penetrate so far, because the only force to help it to penetrate would be gravitation, and that would have to act against the increasing repulsion of heat.

There is another circumstance to be considered which would tend to increase the density of the outer portion of the crust, if there be a crust, and if not, of the outer portion of the earth itself.

When the earth was in the molten liquid state, it is generally supposed to have been surrounded by vapours of a great proportion of the metals and of some of the metalloids, in addition to the vapour of water, air, and other gases, which floated above them higher up in the atmosphere. In that case when the crust began to be formed through cooling, these vapours would be precipitated on the surface and mixed with the half-liquid half-solid matter there, but the proportion of condensed vapours would be very small compared with what they fell upon, and the specific gravity of the mixture would not be great enough to cause it to sink much below the surface, because it would soon meet with matter as dense as itself; consequently we must consider that all these metals would remain near the surface—most likely much nearer to it than the 9 miles which we have as yet descended to—and whatever may have been the proportion of their density it ought to be added to the weights and pressures that have been taken into account above. We believe that it will be shown later on that this estimate of a density of three times that of water at 9 miles deep in the earth is very much lower than it should be ; because, when the pressure upon the matter there came to be greater than its crushing strain, compression would go on more rapidly than shortly afterwards, and it might so be that with a strain of very much less than four times that of crushing, compression would be reduced to its utmost limit. But more of this hereafter.

Having determined densities for the matter composing the earth at 2, 4½, and 9 miles below the surface, that is, to where the mean diameter comes to be 7900 miles, if we divide that diameter into layers of 25 miles each in thickness, compute the volume of each layer or shell, increase the density of each layer as we descend in direct proportion from 3—the density we have fixed for 9 miles deep—to 13·734 times the density of water, at the centre, and multiply the volume of each layer from the surface downwards by its respective average density, we shall find a mass nearly equal to the mass of the earth at the density of water—always taking its mean diameter at 7918 miles, and mean density at 5·66 times that of water, as already premised. These calculations have been carefully carried out, and are represented in detail in Table IV. for future reference. They terminate in a deficiency of over 70,000,000 of cubic miles, a deficiency which would be more than made up by making the central density 13·736 instead of 13·734. Thus we see that if the density of the earth increases regularly from the surface to the centre, and if the densities we have given to the layers between the surface and 9 miles in depth are not greater than those adopted, the central density must be exceedingly near 13¾ times that of water. Of course, if the three surface densities are in reality *less* than those we have adopted, the central density must be greater than 13¾ times that of water. The whole being a result to our calculations which leads us to speculate on what kind of matter there is at the centre of the earth.

We are acquainted with various kinds of rocks, stones and other solid matter that have densities (specific gravities) of 2½ to 3 times that of water, and we have to conceive that a cubic foot of one of these would have to be compressed into a height of 2¾ or 2¼ inches in order to have the density of 13¾ required at the centre, a result which presents us with a substance which it is difficult to imagine or to believe to exist. It may be that the centre of the earth is occupied by the heaviest metals we know, arranged in layers proportioned in thickness to the masses required of them, and that they are laid one over the other according to their densities, or mixed together until a distance from the centre is attained, at which ordinary rocks compressed as highly as their nature would admit of, may exist; but we do not derive much knowledge or satisfaction from such a supposition. An examination of our table of calculations will show that 500 miles in diameter of the central part might be filled up with platinum, the few other rarer and heavier metals, and gold amalgamated with mercury in due proportions. Then there might be a mixture of mercury and lead to 1800 miles in diameter, followed by a mixture of lead and silver to 2400 miles. After that might come a compound of silver, copper, tin, and zinc to 4900 miles, and some compounds of iron might finish the filling process up to 6000 miles in diameter, or thereby; where the known rocks, compressed to half their volume at first, but gradually allowed to expand, might complete the whole mass of the earth. It will be seen, also, that by the time compressed rocks could be used for this filling process, more than 43 per cent. of the whole volume of the earth would be occupied exclusively by pure metals mixed by rule and measure.

It would appear then that the "sorting-out theory"—about which a good deal has been written—whereby, in suns and planets, the metals on account of being heavier fall more rapidly to the centre, and the lighter metalloids remain near the surface—a theory probably got up to get over the difficulty we are in—is not a very happy one, as too much metal would be required for the process, at least for the earth. No doubt it might be applied differently to what we have done by mixing metals with rocks, stones, earth, etc., forming metallic ores—very rich they would doubtless have to be—from the centre outwards; but however disposed it would seem that very much the same quantity would be required to furnish the desired densities up to 6000 miles in diameter, where we have supposed compressed granites, etc., might come into play. Besides, such an arrangement would do away with the whole beauty of the theory; there would be no law to invoke; it would be all pick-and-shovel work.

The sorting-out theory is one of these notions that occur to humanity and are accepted at once, without consideration of what the consequences may be. If it is made to account for the four inferior planets being so much more dense, and of coming so much sooner to maturity—so to speak—than the four superior ones, it is hard to understand why the sun up to the present day almost ranks in low density with the large planets. If that theory holds good, it would be most natural to suppose that the mean density of the sun should be very much greater than that of Mercury. But it appears to be only carried as far as it suits the theorist, and to be there dropped, or rather ignored.

Having been defeated in our attempt to build up or construct an earth solid to the centre by appealing to the metals to make up the weight or density required for the foundation layers, and that even to somewhere about three-fourths of the diameter of the whole structure, we are forced to fall back upon our known rocks, earths, etc., in order to compound out of them the dense material we require, and of course we feel that we have in hand a more hopeless task than we had with the metals. How are we to compress the ever-lasting hills into one-fourth or one-fifth of their volume? Some solution of the difficulty, or mystery, must be found somewhere; but at the same time the mountains of gold, silver, and less precious metals required have shown us how absurd, even laughable, it is to appeal to them.

Let us suppose that we have a cubic foot of matter of any kind of $13\frac{3}{4}$ times the density of water, and that we place it in one of the scales of a balance at the centre of the earth; we shall find that it does not depress the scale one hair-breadth, for the very good reason that it has nowhere to depress it to; it would be already at what may be called the end of gravitation or tendency to fall lower. As it could not get any lower it would have a tendency to fly off anywhere—provided it was free to do so—and drag the scale and balance along with it, in obedience to its own attractive power and the attraction of all the matter of the earth surrounding it, except that the attraction might be so equally distributed all around it that it would not move in any direction. It would, however, be in a state of very unstable equilibrium, and if by some means the attraction were increased a little on one side more than the others, and it were at liberty to do so, it would abandon the centre and fly off in that direction never to return. Now, this being the case, we are forced to consider how a cubic foot of matter, such as the one we are dealing with, could ever have found its way to the centre of the earth; and the law of gravitation, or rather of attraction, does not in any way help us out of the difficulty. We know that we put our cubic foot of extremely dense matter there for an experiment, but we do not know of any process of nature that could place there any equal mass of matter of that density.

Gravitation and attraction are generally used as synonymous terms, more especially gravitation—somewhat after the manner of the likeness between the two negroes, Cæsar and Pompey, the latter being most especial in the likeness—but there is a very appreciable distinction between them, if we want to use each of them in its proper and strict sense. Gravitation implies the conception of a weight of some kind falling to a fixed centre, while attraction gives the idea of two weights, or masses, drawing each other to a common centre, which when properly looked at is a different thing; because the centre may be anywhere between the two, depending entirely on the difference, if any, in the weights of the masses. The confounding of the two, or rather the almost universal adoption of the less correct term, name, expression—whichever it may be called—has been the cause of wrong conceptions being formed of the construction of almost all—probably all—celestial bodies, and of that most absurd expression, *attraction of gravitation*, used by all our most eminent physicists. The *gravitation* of *attraction* might be excused, but putting cause for effect is hardly scientific. A name is nothing as long as what is meant by it is understood and taken into consideration, but that is not always the case, as we shall proceed to show.

The term gravitation may be applied with almost, but not absolutely, perfect strictness to the attraction between the sun and the planets, because the common centres of their attractions and the centre of the sun are so near each other that they may be looked upon as one and the same thing, or point; but it is not so with the attractions of the planets for each other where there is no common fixed centre, or if there is something approaching to it in a far off way, it is constantly varying, so that the term gravitation cannot be strictly applied to them, nor even to the sun, to speak truly. Planets sometimes *gravitate away* from each other and from the sun, otherwise Adams and Leverrier could not have discovered Neptune from the perturbations of Uranus. Neither can it be properly applied to the different masses of matter in the sun or in the earth—although it was no doubt notions connected with the earth that gave rise to the term, from all ponderable matter falling upon it—because *per se* they could have no tendency to fall to the centre, for *at the centre* there is no sufficient attractive force to draw them towards it. Gravitation was a known term long before the days of Newton, who had the glory of enlightening the world by showing that attraction was the cause of it; and, perhaps unfortunately, the name was continued to represent what it in reality does not.

Let us suppose that we have an empty earth to fill up; if we place one mass of matter at London and another at Calcutta, they could have no tendency of themselves to fall to the centre, but if left alone would go for each other in a straight line and meet half-way between the two, provided they were equal in mass, and attraction, not gravitation, would be the proper term to apply to them. But supposing that two equal masses were placed at their antipodes and the four were left to themselves, they would gravitate towards and meet at the centre in the usual meaning of the word, but the force that drew them there would be really that of attraction. We could, however, place four similar and equal masses at the centre, and give the outer ones just and good reason for gravitating or falling down to it, because those at the centre being equally attracted in the four directions might remain stationary there, but would be in a state of unstable equilibrium. We may now suppose that when the masses had just left London and Calcutta to meet the others, a goodly number of other equal masses were added to those at these two places and began to attract the two bound towards the centre, they would prevent the two from proceeding, or at least retard them on their journey inwards. Moreover, the larger numbers at these two places would attract the four masses at the centre with more force than would the two at the antipodes, and would draw the whole of the four away from the centre and outwards towards themselves; but we might also suppose that at the same moment an equal number of equal masses were added to those at the antipodes, which would again equalize the attractions at the four outer posts, and things would continue as they were at the first; with this difference, that the four at the centre would not be able to balance the attractions at the four outer posts, and the consequence would be—seeing that the forces at the four outer stations were equal to each other, and far superior to the four at the centre—that each one of the four at the centre would be drawn away from it towards one of the outer stations—provided the law of attraction acted impartially—and so the centre would be left without any of the masses at it, that is empty. No doubt when the four outgoing masses met the larger ones coming in, they would all then move towards the centre; but the four places where they met would be immensely nearer the places occupied at first by the outer masses than half-way between them and the centre—proportioned, in exact conformance with the law of attraction, to the excess of the numbers of the masses at the outer stations over those at the centre—and they would be moving, all of them together, to a remote and void space. We may now increase the four outer stations to thousands or millions, with the security that the mode of proceeding would be the same with the whole of them; that is, that the first tendency of the masses at each one of the millions of stations would be to draw away the filling we were pouring into the hollow earth—provided we did it equally and impartially all over the hollow—from the centre, and to leave a void there.

We are accustomed to look upon the earth as a solid body in which there are no acting and counteracting forces, no movements of matter from one place to another, similar to those we have been calling into play, and as if there was only one force acting upon its whole mass and driving it to the centre ; we have, in our ideas, got the whole mass so compressed and wedged in that it cannot move, and never has been able to move in any direction except towards the centre, and this is no doubt the case at the present day. We never stop to think with sufficient care how this compression and wedging-in were brought about, and we only accept what we have been accustomed to believe to be facts, and trouble ourselves no more about it ; but there must have been a time, according to any cosmogony we may choose to adopt—even to the vague one that the solar system was somehow made out of a nebula of some kind—when the matter of the earth was neither compressed nor wedged in, nor prevented from moving in any direction towards which it was most powerfully attracted—before superincumbent matter came, so to speak, to have any wedging-in force—and we must go back to that period and study it deeply, if we want to acquire an accurate knowledge of the construction of the earth.

TABLE IV.—CALCULATIONS OF THE VOLUMES AND DENSITIES OF THE EARTH BETWEEN THE DIAMETER SPECIFIED, REDUCED TO THE DENSITY OF WATER.

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
7918	..	259,923,849,377	5·6600	1,471,168,987,476	Total volume of the earth at density of water.
7914	2·0000	393,724,522	1·0000	393,724,522	Density at 7914 miles in diameter. The 2 miles above being at density of water.
7909	2·5000	491,596,266	2·2500	1,106,090,598	
7900	3·0000	883,309,189	2·7500	2,429,097,520	
		1,768,628,977		3,928,912,640	Volume to 9 miles deep at density of water.
7850	3·0679	4,870,723,550	3·0339	14,777,288,178	
7800	3·1359	4,809,069,650	3·1019	14,917,253,147	
7750	3·2038	4,747,808,450	3·1698	15,049,403,225	
7700	3·2717	4,686,939,950	3·2377	15,174,905,476	
7650	3·3397	4,626,464,150	3·3057	15,293,702,541	
7600	3·4076	4,566,381,050	3·3737	15,405,599,748	
7550	3·4756	4,506,690,650	3·4416	15,510,226,541	
7500	3·5435	4,447,392,950	3·5095	15,608,125,558	
7450	3·6114	4,388,487,950	3·5775	15,699,815,651	
7400	3·6794	4,329,975,650	3·6454	15,784,493,235	
7350	3·7473	4,271,856,050	3·7132	15,862,255,885	
7300	3·8152	4,214,129,150	3·7812	15,934,465,142	
7250	3·8832	4,156,794,950	3·8492	16,000,335,122	
7200	3·9511	4,099,853,450	3·9172	16,059,945,934	
7150	4·0191	4,043,304,650	3·9851	16,112,973,361	
7100	4·0870	3,987,148,550	4·0531	16,160,311,788	
7050	4·1549	3,931,385,150	4·1210	16,201,238,203	
7000	4·2229	3,876,014,450	4·1889	16,236,236,930	
		80,329,049,377		285,717,488,335	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
7000	4·2229	80,329,049,377	4·1889	285,717,488,335	
6950	4·2908	3,821,036,450	4·2568	16,265,387,960	
6900	4·3587	3,766,451,150	4·3248	16,289,147,934	
6850	4·4267	3,712,258,550	4·3927	16,306,838,133	
6800	4·4946	3,658,458,650	4·4606	16,318,920,654	
6750	4·5625	3,605,051,450	4·5285	16,325,475,491	
6700	4·6305	3,552,036,950	4·5965	16,326,937,841	
6650	4·6984	3,499,415,150	4·6645	16,323,022,067	
6600	4·7664	3,447,186,050	4·7324	16,313,463,263	
6550	4·8343	3,395,349,650	4·8004	16,299,036,460	
6500	4·9022	3,343,905,950	4·8682	16,278,802,946	
6450	4·9702	3,292,854,950	4·9362	16,254,190,604	
6400	5·0381	3,242,196,650	5·0042	16,224,600,476	
6350	5·1060	3,191,931,050	5·0721	16,189,793,479	
6300	5·1740	3,142,058,150	5·1400	16,150,178,891	
6284	5·214	962,684,511	5·2080	5,013,660,933	
Half volume of the earth		129,961,924,688		518,596,945,467	0·352507 of the whole volume of the earth at density of water.
6250	5·2420	2,129,893,439	5·2080	11,092,485,030	
6200	5·3098	3,043,490,450	5·2759	16,057,151,265	
6150	5·3778	2,994,795,650	5·3438	16,003,588,994	
6100	5·4457	2,946,493,550	5·4118	15,954,833,794	
6050	5·5137	2,898,584,150	5·4797	15,883,371,567	
6000	5·5816	2,851,067,450	5·5477	15,816,866,892	
		146,826,249,377		609,405,243,009	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
6000	5·5816	146,826,249,377	5·5477	609,405,243,009	
5950	5·6495	2,803,943,450	5·6156	15,745,824,838	
5900	5·7175	2,757,212,150	5·6835	15,670,605,255	
5850	5·7850	2,710,873,550	5·7515	15,591,589,223	
5800	5·8533	2,664,927,650	5·8194	15,508,275,967	
5750	5·9213	2,619,374,450	5·8873	15,421,043,199	
5700	5·9892	2,574,213,950	5·9553	15,330,206,336	
5650	6·0572	2,529,446,150	6·0232	15,235,360,051	
5600	6·1251	2,485,071,050	6·0912	15,137,064,780	
5591½	..	412,281,190	6·1591	2,539,281,080	
0·647819 of the whole volume of the earth		168,383,592,967		735,584,493,738	Half mass of the whole earth at density of water.
5550	6·1930	2,028,807,460	6·1591	12,495,628,024	
5500	6·2610	2,397,498,950	6·2270	14,929,225,962	
5450	6·3289	2,354,301,950	6·2950	14,820,330,775	
5400	6·3968	2,311,497,650	6·3628	14,707,597,247	
5350	6·4648	2,269,086,050	6·4308	14,592,038,570	
5300	6·5327	2,227,067,150	6·4988	14,473,263,994	
5250	6·6006	2,185,440,950	6·5667	14,351,135,086	
5200	6·6686	2,144,207,450	6·6346	14,225,958,748	
5150	6·7365	2,103,366,650	6·7026	14,098,025,308	
5100	6·8045	2,062,918,550	6·7705	13,966,990,043	
5050	6·8724	2,022,863,150	6·8385	13,833,349,651	
5000	6·9403	1,983,200,450	6·9064	13,696,775,588	
		194,473,849,377		905,774,812,734	

TABLE IV.—CALCULATIONS—*continued.*

Diameter in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
5000	6·9403	194,473,849,377	6·9064	905,774,812,734	
4950	7·0083	1,943,930,450	6·9743	13,557,554,137	
4900	7·0762	1,905,053,150	7·0423	13,418,813,378	
4850	7·1441	1,866,568,550	7·1102	13,271,675,704	
4800	7·2121	1,828,476,650	7·1781	13,124,988,241	
4750	7·2800	1,790,777,450	7·2461	12,976,152,480	
4700	7·3479	1,753,470,950	7·3140	12,824,887,528	
4650	7·4159	1,716,557,150	7·3819	12,671,453,226	About density of iron
4600	7·4838	1,680,036,050	7·4499	12,516,100,569	
4550	7·5518	1,643,907,650	7·5178	12,358,568,931	
4500	7·6197	1,608,171,950	7·5858	12,199,270,778	
4450	7·6876	1,572,828,950	7·6537	12,037,960,935	
4400	7·7556	1,537,878,650	7·7216	11,874,883,784	
4350	7·8235	1,503,321,050	7·7896	11,710,269,651	
4300	7·8914	1,469,156,150	7·8575	11,543,894,449	
4250	7·9594	1,435,383,950	7·9254	11,375,991,957	
4200	8·0273	1,402,004,450	7·9934	11,206,782,371	
4150	8·0953	1,369,017,650	8·0613	11,036,061,982	
4100	8·1632	1,336,423,550	8·1295	10,864,454,250	
4050	8·2311	1,304,222,150	8·1972	10,690,969,808	
4000	8·2991	1,272,413,450	8·2651	10,516,624,406	
		226,413,449,377		1,147,552,171,299	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
4000	8·2991	226,413,449,377	8·2651	1,147,552,171,299	About density of copper
3950	8·3670	1,240,997,450	8·3331	10,341,355,851	
3900	8·4349	1,209,974,150	8·4010	10,164,992,834	
3850	8·5029	1,179,343,550	8·4689	9,987,742,590	
3800	8·5708	1,149,105,650	8·5369	9,809,800,023	
3750	8·6387	1,119,260,450	8·6048	9,631,012,320	
3700	8·7067	1,089,807,950	8·6727	9,451,577,408	
3650	8·7746	1,060,748,150	8·7407	9,271,681,355	
3600	8·8426	1,032,081,050	8·8086	9,091,189,137	
3550	8·9105	1,003,806,650	8·8766	8,910,390,109	
3500	8·9784	975,924,950	8·9445	8,729,160,715	
3450	9·0464	948,435,950	9·0124	8,547,684,156	
3400	9·1143	921,339,650	9·0804	8,366,132,559	
3350	9·1822	894,636,050	9·1483	8,184,398,976	
3300	9·2502	868,325,150	9·2162	8,002,658,247	
3250	9·3181	842,406,950	9·2842	7,821,074,605	
3200	9·3860	816,881,450	9·3522	7,639,638,697	
3150	9·4540	791,748,650	9·4200	7,458,252,283	
3100	9·5219	767,008,550	9·4880	7,277,377,122	
3050	9·5899	742,661,150	9·5554	7,096,424,353	
3000	9·6578	718,706,450	9·6239	6,916,759,004	
		245,786,649,377		1,320,251,473,643	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
3000	9·6578	245,786,649,377	9·6239	1,320,251,473,643	
2950	9·7257	695,144,450	9·6918	6,737,200,981	
2900	9·7937	671,975,150	9·7597	6,558,275,871	
2850	9·8616	649,198,550	9·8276	6,380,063,674	
2800	9·9295	626,814,650	9·8956	6,202,697,051	
2750	9·9975	604,823,450	9·9635	6,026,158,444	
2700	10·0654	583,224,950	10·0315	5,850,621,086	
2650	10·1334	562,019,150	10·0994	5,675,940,204	
2600	10·2013	541,206,050	10·1674	5,502,658,393	
2550	10·2692	520,785,650	10·2353	5,330,397,363	
2500	10·3372	500,757,950	10·3032	5,059,409,310	
2450	10·4051	481,122,950	10·3712	4,989,822,340	
2400	10·4730	461,880,650	10·4391	4,821,618,293	About density of silver.
2350	10·5410	443,031,050	10·5070	4,654,927,242	
2300	10·6089	424,574,150	10·5750	4,489,871,636	
2250	10·6768	406,509,950	10·6429	4,326,444,747	
2200	10·7448	388,838,450	10·7108	4,164,770,870	
2150	10·8127	371,559,650	10·7798	4,005,338,715	
2100	10·8807	354,673,550	10·8467	3,847,037,595	
2050	10·9486	338,180,150	10·9147	3,691,134,883	
2000	11·0165	322,079,450	10·9826	3,537,269,768	
		255,735,049,377		1,422,103,132,109	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
2000	11·0165	255,735,049,377	10·9826	1,422,103,132,109	
1950	11·0845	306,371,450	11·0505	3,385,557,708	
1900	11·1524	291,056,150	11·1185	3,236,107,805	
1850	11·2203	276,133,550	11·1864	3,088,940,344	
1800	11·2883	261,603,650	11·2543	2,944,165,858	
1750	11·3562	247,466,450	11·3223	2,802,888,487	About density of lead.
1700	11·4242	233,721,950	11·3902	2,662,139,545	
1650	11·4921	220,370,150	11·4582	2,525,045,253	
1600	11·5600	207,411,050	11·5261	2,390,640,503	
1550	11·6280	194,844,650	11·5940	2,259,028,872	
1500	11·6959	182,670,950	11·6620	2,130,308,620	
1450	11·7638	170,889,950	11·7299	2,004,522,025	
1400	11·8318	159,501,650	11·7978	1,881,768,566	
1350	11·8997	148,506,050	11·8658	1,762,143,088	
1300	11·9676	137,903,150	11·9337	1,645,694,821	
1250	12·0356	127,692,950	12·0016	1,532,519,609	
1200	12·1035	117,875,450	12·0696	1,422,709,531	
1150	12·1715	108,450,650	12·1375	1,316,319,764	
1100	12·2394	99,418,550	12·2055	1,213,453,112	
1050	12·3073	90,779,150	12·2734	1,114,168,820	
1000	12·3753	82,532,450	12·3413	1,018,557,725	
		259,400,249,377		1,464,439,812,165	

TABLE IV.—CALCULATIONS—*continued.*

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
1000	12·3753	259,400,249,377	12·3413	1,464,439,812,165	
950	12·4432	74,678,450	12·4093	927,507,290	
900	12·5111	67,217,150	12·4772	838,681,824	
850	12·5791	60,148,550	12·5451	754,569,575	
800	12·6470	53,472,650	12·6132	674,461,229	
750	12·7149	47,189,450	12·6820	598,456,605	
700	12·7829	41,298,950	12·7489	526,516,184	
650	12·8508	35,801,150	12·8169	458,859,759	
600	12·9188	30,696,050	12·8848	395,512,465	
550	12·9867	25,983,650	12·9528	336,561,022	
500	13·0546	21,663,950	13·0207	282,079,794	
450	13·1226	17,736,950	13·0886	232,151,644	
400	13·1905	14,202,650	13·1566	186,858,585	
350	13·2584	11,061,050	13·2245	146,276,856	
300	13·3264	8,312,150	13·2924	110,488,423	
250	13·3943	5,955,950	13·3604	79,573,874	
200	13·4623	3,992,450	13·4283	53,611,816	
150	13·5202	2,421,650	13·4963	32,683,315	
100	13·5981	1,243,550	13·5642	16,867,761	
50	13·6661	458,150	13·6321	6,245,547	About density of mercury.
0	13·7340	65,450	13·7001	896,672	
		259,923,849,377		1,471,098,672,405	
	True volume at density of water .			1,471,168,987,476	
	Deficiency . . .			70,315,071	About $\frac{1}{21,000}$th part.

CHAPTER IX.

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THE INTERIOR OF THE EARTH AND ITS DENSITY—*continued.*

WHEN, according to the nebular hypothesis, the ring for the formation of the earth and moon had been thrown off by the nebula, and had broken up and formed itself into one isolated mass—rotating or not on an axis, as the case may have been —it must have been in a gasiform state. What was its density, more or less, may be so far deduced from Table III., where it will be seen that when it had condensed to about one-half of its volume, it must have had a density of only $\frac{1}{9000}$th part of our atmosphere, and in which each grain of matter would have for its habitat 16 cubic feet of space, or a cube of 2·52 feet to the side. So that, with an average distance from its neighbours of 2½ feet, a grain of matter could not be looked upon as wedged-in in any way, and would be free to move anywhere. Now, supposing this earth-moon nebula to have been in the form of even an almost shapeless mass, and that it was nearly homogeneous—as it could hardly be otherwise after the tumbling about it had in condensing from a flat ring

—its molecules would attract each other in all directions, and as the mass—without having arrived perhaps at the stage of having any well defined centre—would have an exterior as well as an interior, the individual molecules at the exterior would draw those of the interior out towards them, just as much as those at the interior would attract those of the exterior in towards them ; but as the number of those at the exterior would—owing to the much greater space there, being able to contain an immensely greater number—be almost infinitely greater than of those nearer to the central part, the latter would be more effectually attracted, or drawn, outwards than the former would be inwards, and there would be none left at the interior after condensation had fairly begun. The mass would speedily become a hollow body, the hollow part gradually increasing in diameter. But let us go deeper into the matter.

Let us suppose that the whole mass had assumed nearly the form of a sphere. We have already shown that, although the general force of attraction would cause all the component particles of the sphere to mutually draw each other in towards the centre, yet the more powerful tendency of the particles at the exterior—due to their greatly superior number—would at first be to draw the particles near the centre outwards towards them, and that there would consequently be a void at the centre, for a time at least. Of course it is to be understood that each part of the exterior surface would draw out to it the particles on its own side of the centre, just in the same manner as the four masses we placed at the centre were shown to be drawn out by those at London, Calcutta, and their antipodes. Now we must try to find out what would be the ultimate result of this action ; whether it would be to form a sphere solid to the centre, or whether the void at first established there would be permanent.

In order to show how the heat of the sun is maintained by the condensation and contraction of that luminary, Lord Kelvin—in his lecture delivered at the Royal Institution, on Friday, January 21, 1887—described an ideal churn which he supposed to be placed in a pit excavated in the body of the sun, with the dimension of one metre square at the surface, and tapering inwards to nothing at the centre. In imitation of him, we shall suppose a similar pit of the same dimensions to be dug in the spherical mass, out of which we have supposed the earth to have been formed ; only we shall call it a pyramid instead of a pit. This we shall suppose to be filled with cosmic matter, and try to determine what form it would assume were it condensed into solid matter, in conformity with the law of attraction. The apex of our imaginary pyramid would, mathematically speaking, have no dimension at all, but we shall assume that it had space enough to contain one molecule of the cosmic matter of which the sphere was formed. This being so arranged, we have to imagine how many similar molecules would be contained in one layer at the base of the pyramid at the surface of the sphere, and we may be sure that when brought under the influence of attraction, the great multitude of them would have far more power to draw away the solitary molecule from the apex, than the single one there would have to draw the whole of those in the layer at the base in to the centre of the sphere. A molecule of the size of a cubic millimetre would be an enormously large one, nevertheless one of that size placed at the apex of the pyramid would give us one million for the first layer at the base, and shows us what chance there would be of the solitary one maintaining its place at the apex. At the distance of one-twentieth of the radius of the sphere from the centre, the dimension of the base of the pyramid would be one-twentieth of a square metre, and the proportion of preponderance of a layer of molecules there would be as 25 to 1, so that the molecule at the centre would be drawn out almost to touch those of that layer ; at one-tenth of the radius from the centre, the preponderance of a layer over the solitary central molecule would be as 10,000 to 1 ; and so on progressively to 1,000,000 to 1, as we have already said.

Following up this fact, if we divide the pyramid into any number of frusta, the action of attraction will be the same in each of them ; the molecules in the larger end of each will have more power to draw outwards those of the small end, than they will have to draw inwards those of the larger end ; and then the condensed frusta will act upon each other in the same manner as the molecules did, the greater mass of those at the larger end, or base, drawing down, or out—whichever way it may seem best to express it—a greater number of the frusta at the smaller end of the pyramid, until, in the whole of it, a point would be reached where the number of molecules in the various frusta drawn down from the apex would be equal to those drawn up from the base, leaving a part of the pyramid void at each end, because we are dealing with attraction, not gravitation, and there would be no falling to the base or apex, but concurrence to the point, just hinted at, where the outwards and inwards attractions of the masses would balance each other. This point of meeting of the two equal portions of cosmic matter may be called the plane of attraction in the pyramid. The whole pyramid would thus be reduced to the frustum of a pyramid, whose height would be as much more than double the distance from the plane of attraction to its base, as would be required to make the upper part above the plane of attraction equal in volume, or rather in number of molecules, to the lower part. It would be impossible for us to explain how, in a pyramid such as the one we have before us, the action of attraction could condense, and at the same time cram, the whole of the molecules contained in it into the apex end.

We must not, however, forget that there are two sides to a sphere, as well as to a question, and that we must place on the opposite side to the one we are dealing with, another equal pyramid with apex at the centre and base at the surface, at a place diametrically opposite to the first one, and that the tendency of the whole of this new pyramid would be to draw the whole of the first one in towards the centre of the sphere. But in the second, the law of attraction would have the same action as in the first ; the molecules of the matter contained in it near the base would far exceed, in attractive force, those near the apex, and would draw them outwards till the whole were concentrated in a frustum of a pyramid, exactly the same as the one in the first pyramid. And while the whole masses of matter in the two pyramids were attracting each other at an average distance, say, for simplicity's sake, of one-half the diameter of the sphere, the molecules in each of them would be attracting each other from an average distance of one-quarter the diameter of the sphere; their action would consequently be four times more active, and they would concentrate into the frusta as we have shown, before the two pyramids had time to draw each other in to the centre. There would be then two frusta of pyramids attracting each other *towards* the centre with an empty space between them. Here then we have two elements of a hollow sphere, one on each side of the centre, and if we suppose the whole sphere to have been composed of the requisite number of similar pyramids, set in pairs diametrically opposite to each other, we see that the whole mass of the matter out of which the earth was formed must have—by the mutual attractions of its molecules—formed itself into a hollow sphere.

All that has been said must apply equally well whether we consider the earth to have been in a gasiform state, or when by condensation and consequent increase of temperature it had been brought into a molten liquid condition. For up to that time it must have been a hollow sphere, and we must either consider it to be so still, or conceive that the opposite sides have continued to draw each other inwards till the hollow was closed up; in which case, the greatest density would not be at the centre, but at a distance therefrom corresponding to what has been called the plane of attraction of the pyramid. That the opposite sides have not yet met will be abundantly demonstrated by facts that will meet us, if we try to find out what is the greatest density of the earth at the region of greatest mass or attraction, wherever that may be.

Seeing that the foregoing reasoning forces us to look upon the earth as a hollow sphere, or shell, in which the whole of the matter composing it is divided into two equal parts, attracted outwards and inwards by each other to a common plane, or region of meeting, we shall divide its whole volume into two equal parts radially, that is, one comprising a half from the surface inwards, and the other a half from the centre outwards—that is to say, each one containing one-half of the whole volume of the earth. Referring now to our calculations, Table IV., we find that the actual half volume of the earth is comprised in very nearly 817 miles from the surface, where the diameter is 6284 miles, because the total volume at 7918 miles in diameter is 259,923,849,377 cubic miles. This being the case, we cannot avoid coming to the conclusion, after what has just been demonstrated by the pyramids that if one-half of the whole volume is comprehended in that distance from the surface, so also must be one-half of the mass.

But for further substantiation of this conclusion let us return to the table of calculations. There we find that from the surface to the depth of 817 miles—where the diameter would be 6284 miles—which comprehends one-half of the volume—the mass at the density of water is shown to be only 518,596,945,467 miles instead of 735,584,493,738 cubic miles, which is the half of the whole mass of the earth reduced to the density of water. That is, the outer half of the volume gives only 70·5 per cent. of half the mass, while the inner half of the volume gives not only one-half of the mass but 29·5 per cent. more; or, to put it more clearly, the mass of the inner half-volume is 1·84 times, nearly twice as great, as the mass of the outer half-volume. On the other hand, we have to notice that the line of division of the mass into two halves falls at 1163·25 miles from the surface, where the diameter is 5591·5 miles; so that on the outer half of the earth, measured by mass, 64·74 per cent. of the whole volume of the earth contains only one-half of the mass, whereas on the inner portion, measured in the same way, 35·26 per cent. of the same whole contains the other half. All these results must be looked upon as unsatisfactory, or we must believe that two volumes of cosmic matter which at one time were not far from equal, had been so acted upon by their mutual attractions that the one has come to be not far from double the mass of the other; that the vastly greater amount of cosmic matter at the outer part of a nebula has only one-half of the attractive force of the vastly inferior quantity at the centre. This we cannot believe if the original cosmic, or nebulous, matter was homogeneous;

and if it was not homogeneous we have, in order to bring about such result, to conceive that the earth was built up, like any other mound of matter, under the direction of some superintendent who pointed out where the heavier and where the lighter matter was to be placed.

We shall now proceed to find out what would be the internal form, and greatest density of the earth, under the supposition that it is a hollow sphere divided into two equal volumes and masses—exterior and interior—meeting at 817 miles from the surface; but before entering upon this subject we have something to say about the notion of the earth being solid to the centre.

We are forced to believe that, according to the theory of a nucleus being formed at the centre as the first act, the matter collected there must have remained stationary ever since, because we cannot see what force there would be to uniform the nucleus just formed; gravitation, weight falling to a centre, would only tend to increase, condense, and wedge in the nucleus more thoroughly. Attraction, as we have shown, would not allow the matter to get to the centre at all. Convection currents, or currents of any kind, could not be established in matter that was being wedged in constantly. Moreover, when in a gasiform state, it would be colder than when condensed by gravitation to, or nearly to, a liquid or solid state, and heat would be produced in it in proportion to its condensation, that is, gradually increasing from the surface to the centre in the same manner as density, which, when the cooling stage came, would be conducted back to the surface to be radiated into space, but could not be carried—by convection currents—because the matter being heavier there than any placed above it, and being acted upon by gravitation all the time, would have no force tending to move it upwards; and above all, when solidification began at the surface, it is absurd to suppose that the first formed pieces of crust could sink down to the centre through matter more dense than themselves; unless it was that by solidification they were at once converted into matter of the specific gravity of 13·734. Even so the solid matter would not be very long in being made liquid again by meeting with matter not only hotter than itself, but constantly increasing in heat through continual condensation, which would act very effectively in preventing any convection current being formed to any appreciable depth, certainly never to any depth nearly approaching to the centre. If solidification began first at the centre—as some parties have thought might be the case—owing to the enormous pressure it would be subjected to there, before it began at the surface, then, without doubt, the central matter must have remained where it was placed at first, up to the present day. This would suit the sorting-out theory very well, as all the metals would find their way to the centre and there remain ; but judged under a human point of view, it would be considered very bad engineering on the part of the Supreme Architect to bury all the most valuable part of His structure where they could never be availed of ; or that He was not sufficiently fertile in resources to be able to construct His edifice in a way that did not involve the sacrifice of all the most precious materials in it. Man uses granite for foundations—following the good example He has actually given we believe, and are trying to show—and employs the metals in superstructures ; but some people may also think that it was better to keep the root of all evil as far out of man's reach as possible. What a grand prospectus for a Joint Stock Company might be drawn up, on the basis of a sphere of a couple of thousand miles in diameter of the most precious metals, could only some inventive genius discover a way to get at them !

Returning to our pyramids. We know that the centre of gravity of a pyramid is at one-fourth of its height, or distance from the base, and if we lay one of 3959 miles long (the radius of the earth) over a fulcrum, so that 989¾ miles of its length be on one side of it and 2969¼ miles on the other, it will be in a state of equilibrium. This does not mean, however, that there are equal masses of matter on each side of the fulcrum, for we know that the mass of the base part must be considerably greater than that of the apex part, and that it must be counterbalanced by the greater leverage of the apex part, due to its greater distance from the point of support. This being so, in the case of a pyramid consisting of gasiform, liquid, or solid matter, the attractive power of the $989\frac{3}{4}$ miles of the base part would be greater than that of the $2969\frac{1}{4}$ miles of the apex part, and the plane of equal attraction of the two parts would be less than 990 miles from the base of the pyramid. This is virtually the same argument we have used before repeated, but it is placed in a simpler and more practical light, and shows that the plane of attraction in a pyramid will not be at its centre of gravity but nearer to its base, and that it must be at or near its centre of volume. Thus the plane of attraction in one of the pyramids we have been considering of 3959 miles in length, and consequently the radial distance of the region of maximum attraction of the earth, would not be at 990 miles from the base or surface, but at some lesser distance.

Now, if we take a pyramid, such as those we have been dealing with, whose base is 1 square and height 3959, its volume would be the square of the base multiplied by one-third of the height, that is $1^2 \times \frac{3959}{3} = 1319{\cdot}66$, the half of which is 659·83. Again, if we take the plane of division of the volume of the pyramid into two equal parts to be 0·7937 in length on each side, and consequently (from equal triangles) the distance from the plane to the apex to be 0·7937 of the total height of 3959, which is 3142·258; then, as we have divided it into a frustum and a now smaller pyramid, if we multiply the square of the base of this new pyramid by one-third of the height we have $0{\cdot}7937^2 \times \frac{3142{\cdot}258}{3}$, or $0{\cdot}62996 \times 1047{\cdot}419 = 659{\cdot}83$, which is equal to the half-volume of the whole pyramid as shown above. Thus we get 3959 less 3142·258 = 816·74 miles as the distance from the base of the plane of division of the pyramid into two equal parts, which naturally agrees with the division of the earth into the two equal volumes that we have extracted from the table of calculations, where we have supposed the earth to be made up of the requisite number of such pyramids. So that it would seem that we are justified in considering that the greatest density of the earth must be at the meeting of the two half-volumes, outer and inner, into which we have divided it.

Considering, then, that one-half of the volume and mass of the earth is contained within 817 miles in depth from the surface, this half must have an average density of 5·66 times that of water, the same as the whole is estimated to have. Also, as we have seen already, that, taking its mean diameter at 7918 miles, its mass will be equivalent to 1,471,168,987,476 cubic miles, one-half of this quantity, or 735,584,493,738 cubic miles will represent the half-volume of the earth reduced to the density of water. With these data let us find out what must be the greatest density where the two half-volumes meet, supposing the densities at the surface and for 9 miles down to remain the same as in the calculations we have already made, ending with specific gravity of 3 at 7900 miles in diameter.

Following the same system as before when treating of the earth as solid to the centre, and using the same table of calculations for the volumes of the layers : If we adopt a direct proportional increase between densities 3 at 7900 miles and 8·8 at 6284·5 miles in diameter, multiply the volumes by their respective densities, and add about 31 per cent. of the following layer, taken at the same density as the previous or last one of the number, we shall find a mass (see Table V.) of 735,483,165,215 cubic miles at the density of water, which is as near the half mass 735,584,493,738 cubic miles as is necessary for our purpose. It would thus appear that if the earth is a hollow sphere, its greatest density in any part need not be more than 8·8 times that of water, instead of 13·734 times, if we consider it to be solid to the centre.

Let us now try to find out something about the inner half-mass of the earth, and the first thing we have got to bear in mind is, that where it comes in contact with it, its density must be the same as that of the outer half-mass at the same place, and continue to be so for a considerable distance, varying much the same as the other varies in receding from that place, and diminishing at the same rate as it diminishes. This being the case—and we cannot see how it can be otherwise—if we attempt to distribute the inner half-mass over the whole of the inner half-volume, and suppose that its density decreases from its contact with the outer half—where it was found to be 8·8 times that of water—to zero at the centre, in direct proportion to the distance; then, it is clear that at half the distance between that place and the centre, the density must be just 4·4 times that of water. Now, if we divide the outer moiety of the inner half-mass of the earth—that is, the distance between the diameters of 6284·5 miles and 3142·25 miles—into layers of 25 miles thick each, take their volumes from Table IV., and multiply each of them by a corresponding density, decreasing from 8·8 to 4·4, we shall obtain a mass far in excess of the whole mass corresponding to the inner half of the earth. This shows that a region of no density would not be at the centre but would begin at a distance very considerably removed from it. It is another notice to us that the earth must be a hollow sphere. But why should there be a zero point or place of no density? And what would a zero of no density be? It would represent something less than the density of the nebulous matter out of which the earth was formed; and all that we have contended for, as yet, is that there is a space at the centre where there is no greater density than that corresponding to the earth nebula; but we must now go farther.

If the earth is a hollow sphere, it must have an internal as well as an external surface. But how are we to find out what is the distance between these two surfaces? Let us, to begin, take a look at the hollow part of the sphere. From the time of Arago it began to be supposed that there is a continual deposit of cosmic matter upon the earth going on, and since then it has been proved that there is a constant and enormous shower of meteors and meteorites falling upon it. But although this is the case on the exterior surface, it may be safely asserted that on the interior surface, where the supply of cosmic matter must have been limited from the beginning, there can be no continual deposit of such matter going on now; nor can there have been from, at least, the time when the earth changed from the form of vapour to a liquid state. We may, therefore, be sure that there is no undeposited *cosmic* matter of any kind in the hollow of the sphere, and that, as far as it is concerned, there is an absolute vacuum.

TABLE V.—CALCULATIONS OF THE VOLUMES AND DENSITIES OF THE OUTER HALF OF THE EARTH—TAKEN AS A HOLLOW SPHERE—AT THE DIAMETERS SPECIFIED, AND REDUCED TO THE DENSITY OF WATER. With mean diameter of 7918 miles. Diameter of half-volume at 6284·5 miles, and density there of 8·8 times that of water.

Diameters in miles.	Densities.	Volumes in Cubic Miles.	Averages of Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
7918	..	129,961,924,377	..	735,584,493,738	Half-volumes of the earth actual and at density of water.
7914	2·0000	393,724,522	1·0000	393,724,522	Density at 7914 miles in diameter. The 2 miles above being at density of water.
7905	2·5000	491,596,266	2·2500	1,106,090,598	
7900	3·0000	883,309,189	2·7500	2,429,097,520	
		1,768,628,977		3,928,912,640	Volume to 9 miles deep at density of water.
7850	3·1823	4,870,723,550	3·0912	15,056,380,638	
7800	3·3625	4,809,069,650	3·2724	15,737,199,523	
7750	3·5437	4,747,808,450	3·4531	16,394,666,359	
7700	3·7250	4,686,939,950	3·6343	17,023,745,860	
7650	3·9062	4,626,464,150	3·8156	17,652,736,611	
7600	4·0875	4,566,381,050	3·9969	18,251,368,419	
7550	4·2688	4,506,690,650	4·1781	18,829,404,185	
7500	4·4500	4,447,392,950	4·3594	19,387,964,826	
7450	4·6312	4,388,487,950	4·5486	19,926,368,386	
7400	4·8125	4,329,975,650	4·7219	20,445,712,022	
7350	4·9938	4,271,856,050	4·9031	20,945,337,398	
7300	5·1750	4,214,129,150	5·0844	21,426,318,250	
7250	5·3562	4,156,794,950	5·2656	21,888,019,489	
7200	5·5375	4,099,853,450	5·4469	22,331,491,757	
7150	5·7187	4,043,304,650	5·6281	22,756,152,901	
7100	5·900	3,987,148,550	5·8093	23,162,542,072	
		72,521,649,777		315,144,321,336	

TABLE V.—*continued.*

Diameters in miles.	Densities.	Volume in Cubic Miles.	Averages of Densities.	Volume at Density of Water in Cubic Miles.	Observations.
7100	5·9000	72,521,649,777	5·8093	315,144,321,336	
7050	6·0813	3,931,385,150	5·9907	23,551,749,018	
7000	6·2625	3,876,014,450	6·1719	23,922,373,584	
6950	6·4438	3,821,036,450	6·3532	24,275,808,774	
6900	6·6250	3,766,451,150	6·5344	24,611,498,395	
6850	6·8062	3,712,258,550	6·7156	24,930,043,518	
6800	6·9875	3,658,458,650	6·8968	25,231,657,617	
6750	7·1688	3,605,051,450	7·0782	25,517,275,173	
6700	7·3500	3,552,036,950	7·2594	25,785,657,035	
6650	7·5312	3,499,415,150	7·4406	26,037,748,365	
6600	7·7125	3,447,186,050	7·6218	26,273,762,636	
6550	7·8938	3,395,349,650	7·8032	26,494,592,389	
6500	8·0750	3,343,905,950	7·9844	26,699,082,667	
6450	8·2562	3,292,854,950	8·1656	26,888,136,380	
6400	8·4375	3,242,196,650	8·3468	27,061,966,998	
6350	8·6188	3,191,931,050	8·5282	27,221,426,381	
6300	8·8000	3,142,058,150	8·7094	27,365,441,252	
6284½	8·8000	962,684,511	8·8000	8,471,623,697	
		129,961,924,688		735,484,165,215	
True half-volume at density of water .				735,584,493,738	
Deficiency				100,328,522	

As to how far the internal surface is from the centre, it may be possible to designate a position, or region, from which it cannot be very far distant, although we can never expect to be able to point out exactly where it is. Going back to the time when the whole earth was in a molten liquid state, and just before the outer surface began to become solid, it is certain that the interior surface must have been in the same liquid condition, whatever may have been the condition of the mass of matter between the two surfaces, owing to the pressure of superincumbent matter; nay, we may be sure that whatever may be its state now, it continued liquid long after the other became solid, because it had no outlet by which to get rid of its melting heat by radiation, nor weight of superincumbent matter to consolidate it; and it would always be much hotter than the outer surface. At that time we have every reason to believe that the outer surface was at least as dense as it is now, there being no water upon it to lower its average density, as is the case at the present day; and we have equal reason to consider that the density at the inner surface, whether liquid or solid, is now at least equal to what the outer surface was then. Duly considering, therefore, the absence of water from the interior surface, we shall suppose that the first layer of 25 miles thick upon it will have an average density of 2½ times that of water, terminating at 3 times, which is the density we have taken for the outer surface at 9 miles deep. But there is another contingency, which it will be necessary to take into consideration before going any farther.

It has been understood—as it is certainly the truth—in the calculations made with respect to the outer half of the mass of the earth, that the increase of density in descending was due to the pressure of the superincumbent matter, caused by the attraction for it of the inner half, as well as that of the whole of both the outer and inner halves on the other side of the hollow interior. In the case of the inner half we have now to consider that the attraction of the outer half alone would be the effective agent, and that the superincumbent pressure—that is, of course, the pressure acting from the centre outwards—would be interfered with, or perturbed, by the attraction of the mass on the other side of the hollow interior, so that it would not exert its full power in that direction. But that does not mean that the density would be in any way diminished. The attractions of the planets for each other perturb them in their revolutions around the sun, accelerating or retarding each other, but do not increase or diminish their density or mass; only it will lead us to expect that the same depth of 817 miles will not produce the same amount of pressure outwards at the meeting of the two halves as it does inwards, and that to obtain an equal pressure a greater depth will be required. We believe that an expert mathematician, taking as bases two opposite pyramids in a sphere, similar to those we have used in a former part of our work, could point out, with very approximate accuracy, what ought to be the distance of the inner surface of the shell from the centre—provided a maximum density were determined for the earth—but that goes beyond our powers, and we shall limit ourselves to the use of our own implements; which will cause us to depart from the statement we have made, that the density of the inner half must decrease from the place of meeting of the two halves, at the same rate as the outer half had increased. It must decrease much more rapidly than the other increased. All this premised, and having established a density of 3 for the interior surface, we may proceed to calculate where that surface ought to be, so as to give for the interior half of the earth a mass equal to 735,584,493,738 cubic miles of water.

If we begin our operations with a density of 8·8 times that of water at the meeting of the two halves of the shell, and diminish it for any considerable distance at the same rate as it increased when we were finding the mass of the outer half, that is 0·1812 for each layer, we soon find that before we could make up the whole mass of the inner half of the shell, the density would be decreased to at least that of water, which cannot be, as there can be no liquid or solid matter of any kind of so low density anywhere in the interior half of the shell. Furthermore, if we decrease it at the same rate as the volumes of the different layers of the earth decrease as they approach the centre, it involves a mass of calculation that serves no useful purpose, as such calculations bring no contingent of satisfaction with them ; because all the densities with which we are dealing have to be brought to a rational form before we can frame a proper approximate idea of what the interior construction of the earth is, as will be seen hereafter ; and because it takes no account of the perturbation—above alluded to—produced by the attraction of the matter on the opposite side of the hollow. But, in order to get such a result as we can with our limited powers, if we begin with a density of 8·8 at the diameter of 6284·5 miles and fix the density of 3—which we have adopted above —at the diameter of 3200 miles, we shall get a mass somewhat less than one-half of the earth ; and with a density of 2·91 at 3150 miles diameter we get a mass of 735,713,884,116 cubic miles of water, which is rather greater than one-half of the mass required (see operations of Table V.). This density of 2·91 reduced to 2·5, as we mentioned, might be done when we were fixing the number 3, would make very little difference on the resulting mass, compared with what we have been in quest of.

Here we may state that we found that, had the calculations been made with documents of density proportioned to the decrease of the volumes of the layers of the earth as they approached the centre, the density would have been reduced to 2·25 at 3150 miles in diameter ; which tends to show that should that process be considered to bc more accurate, it would not have made any great difference on the result.

With all, we may consider that it has been demonstrated, that the greatest density of the earth is not necessarily greater at any part of its interior than 8·8 times that of water.

Table VI.—Calculations of the Volumes and Densities of the Inner Half of the Earth, on the same Data as those for the Outer Half.

Diameters in Miles.	Volumes in Cubic Miles.	Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
6284½	129,961,924,688		735,584,493,737	Half-volumes of the earth.
6250	2,129,893,439	8·800	18,743,062,263	
6200	3,043,490,450	8·705	26,493,584,367	
6150	2,994,795,650	8·610	25,785,090,546	
6100	2,946,493,550	8·515	25,089,392,578	
6050	2,898,584,150	8·420	24,406,078,543	
6000	2,851,067,450	8·325	23,735,136,521	
5950	2,803,943,450	8·230	23,076,454,654	
5900	2,757,212,150	8·135	22,429,920,840	
5850	2,710,873,550	8·040	21,795,423,342	
5800	2,664,927,650	7·945	21,172,850,179	
5750	2,619,374,450	7·850	20,562,089,432	
5700	2,574,213,950	7·755	19,963,029,182	
5650	2,529,446,150	7·660	19,375,557,509	
5600	2,485,071,050	7·565	18,799,562,493	
5550	2,441,088,650	7·470	18,234,932,216	
5500	2,397,498,950	7·375	17,681,554,755	
5450	2,354,301,950	7·280	17,139,318,196	
5400	2,311,497,650	7·185	16,608,110,615	
5350	2,269,086,050	7·090	16,087,820,094	
5300	2,227,067,150	6·995	15,578,334,714	
5250	2,185,440,950	6·900	15,079,542,555	
5200	2,144,207,450	6·805	14,591,331,697	
	56,339,575,889		442,428,177,291	

TABLE VI.—*continued.*

Diameters in Miles.	Volumes in Cubic Miles.	Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
5200	56,339,575,889	6·805	442,428,177,291	
5150	2,103,366,650	6·710	14,113,590,222	
5100	2,062,918,550	6·615	13,646,206,207	
5050	2,022,863,150	6·520	13,189,067,738	
5000	1,983,200,450	6·425	12,742,062,891	
4950	1,943,930,450	6·330	12,305,079,748	
4900	1,905,053,150	6·235	11,878,006,390	
4850	1,866,568,550	6·140	11,460,730,897	
4800	1,828,476,650	6·045	11,053,141,349	
4750	1,790,777,450	5·950	10,655,125,828	
4700	1,753,470,950	5·855	10,266,572,412	
4650	1,716,557,150	5·760	9,887,369,184	
4600	1,680,036,050	5·665	9,517,402,223	
4550	1,643,907,650	5·570	9,156,565,611	
4500	1,608,171,950	5·475	8,804,741,426	
4450	1,572,828,950	5·380	8,461,819,751	
4400	1,537,878,650	5·285	8,127,688,665	
4350	1,503,321,050	5·190	7,802,236,249	
4300	1,469,156,150	5·095	7,485,350,584	
4250	1,435,383,950	5·000	7,176,919,750	
4200	1,402,004,450	4·905	6,876,831,827	
4150	1,369,017,650	4·810	6,584,974,897	
4100	1,336,423,550	4·715	6,301,237,038	
4050	1,304,222,150	4·620	6,025,506,333	
4000	1,272,413,450	4·525	5,757,670,861	
	96,451,524,689		671,704,075,372	

TABLE VI.—*continued.*

Diameters in Miles.	Volumes in Cubic Miles.	Densities.	Volumes at Density of Water in Cubic Miles.	Observations.
4000	96,451,524,689	4·525	671,704,075,372	
3950	1,240,997,450	4·430	5,497,618,693	
3900	1,209,974,150	4·335	5,245,237,939	
3850	1,179,343,550	4·240	5,000,416,652	
3800	1,149,105,650	4·145	4,763,042,919	
3750	1,119,260,450	4·050	4,533,004,823	
3700	1,089,807,950	3·955	4,310,190,441	
3650	1,060,748,150	3·860	4,094,487,859	
3600	1,032,081,050	3·765	3,885,785,163	
3550	1,003,806,650	3·670	3,683,970,405	
3500	975,924,950	3·575	3,488,931,696	
3450	948,435,950	3·480	3,300,557,106	
3400	921,339,650	3·385	3,118,734,715	
3350	894,636,050	3·290	2,943,352,605	
3300	868,325,150	3·195	2,774,298,854	
3250	842,406,950	3·100	2,611,461,545	
3200	816,881,450	3·005	2,454,728,757	
3150	791,748,650	2·910	2,303,988,572	
	113,596,348,539		735,713,884,116	
			735,584,493,738	True half-volume
			129,390,378	Excess

CHAPTER X.

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Inquiry into the Interior Construction of the Earth—*continued.*

It may be well to revert here to the experiment we made of putting a cubic foot of rock, of specific gravity 13·734 in the scale of a balance at the centre of the earth, where we saw that it could not depress the scale one hairbreadth, and make the same experiment by placing a cubic foot of rock of 8·8 specific gravity in the same scale, at what we have called the region of greatest density of the earth, that is, at 817 miles from its surface. Here, also, we shall find that the scale is not depressed for the very same reason as in the former case, that is because it had nowhere to be depressed to; and it might be argued that for the same reasons advanced formerly there can be no matter at that place, but the cases are entirely different. In the first case, there is nearly the whole mass of the earth drawing the matter away from the centre were it at liberty to move; whereas, in the second case, the meeting of the two halves of the shell, at the region where there is the greatest mass of matter, is also the meeting place of the action of attraction in its greatest force; the place to which matter is attracted from all sides, remains stationary, and it is held there both by attraction and weight of superincumbent matter or gravitation. The attraction of the whole earth acts as if it were concentrated at its centre, but that is for external bodies. That kind of attraction on the inner half of the shell would be far inferior to that outwards of the outer half, owing to its greater distance and conflicting nature, and would perturb, as we have said, but not do away with it. The same could not occur at the centre, because it is not the centre of the mass, that is, it is not the place where the greatest quantity of matter existed originally, or is now to be found, and consequently never was, nor can now ever be, the actual centre of interior attraction.

It has been said when treating of the earth as being solid to the centre, that it is not easy to comprehend what may be the nature of the rocks we are acquainted with, when compressed to one-fourth or one-fifth of their volume, and we do not find ourselves much better off when we contemplate them as reduced to one-third or one-fourth of their bulk, that is, when a cube of one foot is reduced to three or four inches in height, as would be the case with it at a maximum density of 8·8 times that of water when placed at a depth of 817 miles from the surface of the earth. We find, therefore, the idea thrust upon us that there may be a limit to density, perhaps not an absolute limit, but a practical one; in which case, the greatest density of the earth may not greatly exceed 5·66 times that of water. For, if we conceive that it increases to its maximum at 100 miles from the surface, and continues nearly uniform thereafter, a little calculation will show that the greatest density of the outer half of the shell need not much exceed 6 times that of water; and, of course, the same will be the case with the inner half should its density be almost uniform till 100 miles from the inner surface is reached. It might even so happen that at a depth of 25 to 30 miles the practical limit might be reached; for a column of granite of one foot square and 25 miles high would weigh, and exert a pressure upon its base of 10,000 tons, a pressure equal to nearly fifteen times what would be sufficient to crush it into powder; in which case the greatest density of the earth might not much exceed the 5·66 that we are accustomed to think of—without thinking.

It may be deemed absurd to think that there is even a practical limit to the density of matter, but on the other hand it is much more absurd to suppose that there is not an absolute limit to it. We cannot conceive of density being other than the result of compression, and we cannot believe that matter can be compressed more and more continually for ever. There must be some end to compression. Perhaps it was the difficulty in conceiving of rock being compressed to so small a fraction of its volume as would enable it to take its place at the centre of the earth—where it has been said that, "it must weigh like lead"—that originated the idea of its centre being occupied by the metals, arranged as they would be in a rack in a store, the heaviest pieces at the bottom of the rack, and the lighter ones higher up.

When fairly looked at, density would really seem to have a limit, except in so far as it may be combined with heat. We know that water is compressed 0·00005 part of its volume for every atmosphere of pressure to which it is subjected. But 0·00005 for round numbers, is in fractional numbers $\frac{1}{20\,000}$; therefore a pressure of 20,000 atmospheres would compress a cubic foot of water into $\frac{1}{20{,}000}$ of a foot in height, or practically into nothing. We know, also, that as a column of water 33·92 feet high balances one atmosphere, one mile in height will be equal to 155·66 atmospheres, and 20,000 atmospheres will produce a pressure equal to a column of water 128 miles high; therefore, a cubic foot of water, subjected to such a pressure, would be compressed into virtually nothing. Again, supposing that we have a column of liquid rock, of 2½ times the density of water, of the same height of 128 miles, we should have a pressure of 2½ times that of the column of water; and as we have no reason to believe that granite in a liquid state has to obey a different law of compression to the one obeyed by liquid ice; then a column of granite 51 miles high would be sufficient to squeeze its own base, not only off the face of the earth but out of the bowels thereof. It will be seen, therefore, that at 100 miles deep from the surface, the density of the earth might well be equal to not only 5·66 times the density of water but to a great deal more; and that our estimate of 3 times the density of water, at 9 miles deep, was far within the mark.

The authors of text-books on the strength of materials tell us that "the Modulus of Elasticity of any material, is the force that would lengthen a bar of that material of 1 inch square to double its length, or compress it till its length became zero; supposing it possible to stretch or compress the bar to this extent before breaking." This is neither more nor less than a counterpart of the law of gases, upon which the air thermometer is constructed, applied to solid matter, and may be used in the same manner. But we can never produce a perfect vacuum, and so annihilate a gas and temperature; neither can we annihilate matter, nor easily reduce it to one half of its volume. Now, we have seen, a little way back, that a column of granite 25 miles high would exert a pressure at its base 15 times as great as would crush it to pieces; so that a column of 25 ÷ 15, or 1·66 miles high would destroy the elasticity of the material, because, when crushing takes place, all elasticity is gone. We cannot, there-force, get much satisfaction out of any calculations made upon the theory of the strength of materials; still, by them, we can make more plain the absurdity of any notion of the indefinite compressibility of matter. But if, in the face of contravening its conditions, we follow the reasoning used for the formation of the theory, and take the modulus of elasticity for granite as 2,360,000 feet, then the same modulus would compress a bar of granite of 1 inch square in section till its height became zero. And as that length is equal to 447 miles, at that depth from the surface of the earth, granite or any other rock or stone of a similar nature would be compressed out of existence by the weight of the superincumbent matter.

Thus we have arrived at two measures of force which would compress to zero the rocks that are known upon the earth. One where rocks are looked upon as in a molten, liquid state, and analogous to water, where the force is equal to that exerted by a column of the material 51 miles high;

and the other where the column requires to be 447 miles high. In either case the same method of calculation will show that columns one-half of these heights, will compress the material into at least one-half of its volume—that is half-way between what it is at the surface and would be at the specified depths—and consequently into double its density. So we find in the one case that the density of the earth ought to be about 5·66 times that of water at a depth of 25½ miles; and, in the other, at somewhere less than 225 miles deep. But, before proceeding to use and reason upon these depths, we must recall to mind that the calculations from which we have derived them, in the second case, have been made in violation of the theory that was adduced for the purpose, and that in consequence the latter depth must be excessive. For, were we to erect a structure of any kind, calculating the stresses it would have to bear, under the same violation of the theory, we should inevitably find that the structure would give way under the strains that would be brought upon it; that is the columns 25½ and 225 miles high would compress the same kind of matter composing them into very far below one-half of its volume.

This premised, let us go back to our layers of 25 miles thick with their respective volumes. Nine of them counted from the diameter of 7900 miles inwards, will be equal to 225 miles and will bring us to 234 miles deep, which at the same time that it leaves us the same volume and mass that we have always retained for the first 9 miles in depth, will facilitate our calculations considerably without making any appreciable difference in them. We shall then have to find for the 9 layers 9 corresponding densities increasing from 3 to 5·66, and if we multiply these together respectively, and add the numbers of the volumes and masses of the outer 9 miles in depth, we shall get, at the diameter of 7450 miles, a simple volume of 43 418,587,327 cubic miles, and mass volume of 195,312,523,450 cubic miles. Deducting this latter sum from 735,584,493,738 cubic miles, which represents the half mass of the earth at the density of water, we have a remainder of 540,272,970,288 cubic miles. On the other hand we find that the simple volume of the earth comprehended between the diameters of 7450 and 6284·5 miles is 86,543,337,361 cubic miles; so that if we divide 540,272,970,288 by this sum, we find that a density of 6·24 times that of water over the whole intervening space—between the two diameters just cited—will make up the whole half-volume, at the density of water, from the surface of the earth to the diameter of 6284·5 miles. Then, for the inner half-mass:—If we multiply the simple volume between the diameters of 6284·5 miles, and 3150 miles, which is 113,596,348.539 cubic miles by 6·24, we get 708,841,214,870 cubic miles at density of water; and if from there we run down the density to 3 at 2700 miles in diameter we get 27,400,652,354 cubic miles, which added to the last mentioned amount gives 736,241,867,224 cubic miles, somewhat in excess of the inner half-mass of the earth at density of water. Thus we see that in order that the average density of the earth of 5·66 may be made up, there is no necessity for appealing to matter of any kind with a density of more than 6·24 times of water. And there is still something else of importance to be taken into consideration before we can bind ourselves to a density even so great as that.

We have said, a few pages back, that there can now be no undeposited cosmic matter in the interior of the hollow earth, and that as far as such matter is concerned the hollow part may be a perfect vacuum. This is not absolutely true, for gases may be cosmic matter, just the same as any others of the elements out of which the earth is formed, but what is generally meant by cosmic matter is solid—at least, we have always looked upon it in that light—and all solid matter must have been deposited upon the interior surface at an immeasurably long period of time before the nebula forming the earth came to have even the density of water; certainly before it came to be in a molten liquid state; and we did not want to introduce any posterior evolutions in order not to complicate our calculations, and also to obtain some tangible bases to which the consequences of these evolutions might be applied. But as we have now both form and density to work upon we may take them into account, and it will be found that neither of these two bases will be very materially altered by them.

When the earth was in a molten liquid state, it is believed —as we have said on a former occasion—to have been surrounded by a dense atmosphere, composed of gases and vapours of metals, metalloids, and water, and we have no reason to doubt that the hollow of the sphere was filled with a similar atmosphere, only the vapour of water would, most probably, be dissociated into its elements of oxygen and hydrogen. Also we have every reason to believe that even at the present day gases are being produced in the interior, one part of which find their way to the surface and are dissipated into the atmosphere in the same manner as the gases from the chimney of a furnace; and another part into the interior, where they could not escape but would be stored up in the hollow. Thus at the present day there may be an atmosphere there, composed near the surface of vapours of the elements with gases above them, so to speak, at a very high degree of pressure. These gases could not have gone on accumulating always, but must have found an exit in some particular place, or places, when the pressure exceeded the resistance, or when this was diminished by some convulsion such as an earthquake; but we do not want to define too much, or make more suppositions on this point than what present themselves to us in a reasonable way. All that we need say is, that the resisting power of some thousands of miles of solid, or even viscous, matter must be enormous, and the pressure necessary to force its way through it must have been equal to many thousands of atmospheres. We know that a pressure of 773·4 atmospheres condenses air to the density of water, and it must be the same with any similar gas; so we have only to suppose that the pressure is 4827 atmospheres—which is equal to 773·4 multiplied by 6·24—in order to bring the whole of the gases, and vapours of elements, in the hollow to the same density of 6·24 times that of water, which we have shown need not be exceeded in any part of the earth. And such being the case, we can place the division between solid and gasiform matter in any point of the radius that may seem to us reasonable, only we must always have as much solid matter in the inner as in the outer half-mass of the earth.

Following nearly the result we have obtained in another way, by placing the division of the hollow part at 3000 miles in diameter, the volume of which is 14,137,200,000 cubic miles, and multiplying this by 6·24, we get a mass equal to 88,216,128,000 cubic miles at density of water, composed of vaporous and gaseous matter in the hollow centre, and consequently much greater than is required to make up the total mass of the earth at the density of water ; which shows that the density of the mass between the diameters of 7450 and 3000 miles must be less than 6·24 times that of water. How much less is very easily found, by dividing the surplus of 88,216,128,000 cubic miles over the whole volume between 7450 miles in diameter and the centre, because in this way we shall include the whole mass arising from both solid and gasiform matter. This whole volume—that of a globe 7450 miles in diameter—is 216,505,262,050 cubic miles, which, divided by the surplus gives the amount 0·407 as the density to be deducted from 6·24 on its account, and therefore the greatest density of any part of the earth need not be over 5·833 times that of water.

This result derived from our operations will be acknowledged, we doubt not, to be much more satisfactory, we might say, more comprehensible, than to have to believe that our known rocks and stones could be compressed till they were 13·734 or even 8·8 times heavier than water.

At first sight 4827, say 5000, atmospheres or 75,000 lb. on the square inch, appears to be an enormous pressure, but it is nearly almost as nothing compared to the pressures we have been dealing with. A column of granite 1 mile high would exert a pressure upon its base of 6050 lb. per square inch, and one of 25 miles high of 151,200 lb., or double the number of atmospheres we have applied to the gases in the hollow of the earth. If we take a column 225 miles high, such as we considered to be the least that would be necessary to compress granite into one-half of its volume, we get 1,360,860 lb. per square inch, or over 90,000 atmospheres of pressure; and if we go into thinking of columns of 447 and 817 miles—this last being the depth from the surface of the division of the matter of the earth into two equal portions—we could have gases compressed to 174,600 and 326,700 atmospheres or, dividing the numbers by 773·4, 222 and 422 times the density of water; so there is no cause to stumble over high pressure. With even 10,000 atmospheres, more than double the number assumed, we should have gases as heavy as the material we found at the centre of the earth, when we were looking upon it as solid to the centre—which was 13·734 times the density of water—and so get rid of burying the precious metals where they would be "matter in the wrong place," and according to D'Israeli's definition, justly entitled to the epithet applied to them, sometimes, by people who have never been blessed with a superabundant supply of them. At the same time, we find out what we knew before, viz. that we may have gases heavier than the heaviest metals and as rigid as steel, if we can only find a vessel strong enough to compress them in, along with the means of doing it; and also that the thousands of miles of highly compressed matter, between the hollow centre and the surface of the earth, are far more than sufficient to imprison gases of far, very far, greater elasticity than our modest measure of 5000 atmospheres. And we hope to be able to show presently good reason for believing that the gases compressed in the hollow, at what may really be considered as very high pressures, have had, and may probably still have, a very important part to play in the evolution of the earth.

We have just seen that the pressure produced by a column of granite 1 mile high would be 6050 lb. per square inch, consequently one of double the height, or 2 miles, would exert a pressure of 12,100 lb. per square inch at its base, equal to the crushing strain of the very strongest granite we know, while at the same time that strain would not amount to one-sixth of 4827 atmospheres; so that if the gases in the hollow of the earth were at a pressure of only 800 atmospheres, their pressures would be able to crush granite of that class to pieces, and therefore the estimate of specific gravity of 3 for the density of the interior surface—which we made at the beginning of our calculations for the hollow sphere—cannot be looked upon as by any means exaggerated.

We might now reform our calculations of the two halves of the interior of the earth, giving a more rational and curve-like form to the densities, under the supposition that at much less distance than 234 miles from the surface, matter might be compressed to its utmost limit; but as, according to our demonstration, the solid matter of the earth must have been divided into two equal parts at the place where the greatest mass was, long before it could have been condensed into a state to compress gases; and as the total mass of solid matter must, in order to make up the total mass of the earth, depend to some extent on the mass of imprisoned gases; we are unable to make any reform much different to what our calculations show. Besides, as the difference between average densities of 5·66 and 5·67 makes a difference of 2,600,000,000 cubic miles on the mass of the earth reduced to the density of water, very approximate accuracy cannot be attained in any calculations.

What is meant by a limit to density except in so far as it is combined with heat, is that whatever density may be given to matter by compression when it is in a heated state, a greater density will be found in it when it is deprived of that heat; that whatever may be the density of any part of the interior of the earth in its present state, that density will be increased when the earth becomes cooled down to the temperature derived from the heat of the sun, or to absolute zero of temperature, if such there be, on account of shrinking in cooling; and that therefore there can be no absolute limit to density as long as there is any heat in matter.

It may not be unnecessary for us to recognise now that the weight of a column of granite would decrease as the depth increased, for the force of gravitation would be diminished by having a part of the attraction of the earth above instead of below it; but at 100 miles in depth the diminution would be only about one-eighth—if distance is taken into account—of the 817 miles down to the plane of greatest density, and

$\frac{1}{2200}$th part if the mass left above is considered ; differences that would make extremely little alteration on our calculations.

It will not be out of place either to take a look at what may be the temperature of the interior of the shell, and of the gases shut up in the hollow part of the earth ; and we have not much to say on the subject, because we shall not depart from the system we have followed up till now, with considerable strictness, of not theorising or speculating on what may be ; but will restrict our observations to theories that have been very generally adopted by astronomers, geologists, and scientists in general. The air thermometer will be of no use to us, for whatever may have been the temperature when the earth was in the process of formation, it must have diminished very greatly during the cooling process it has undergone since, and we know that gases heated in a closed vessel in such manner that pressure and temperature will agree to the theory on which the air thermometer is constructed, may be cooled down afterwards to almost any degree required, and the relation between temperature and pressure destroyed thereby. At one time it was thought that the earth had only a solid crust, and that, under it, the whole of the interior was in a molten liquid state. Then some physicists thought that, through pressure of superincumbent matter, solidification must have begun at the centre ; others that it began almost simultaneously at the surface and centre, and that there may still be a liquid mass between the two solidifications—this is repeating what we have said before, but it is done only to bring it to mind. We, at present at least, do not want to have anything to do with any of these theories, only we believe that we have shown in an indisputable manner that there could be no solidification at the centre, because there could be no matter there capable of being solidified—gases could not be solidified under such pressure, and at all events heat, as there must have been there. We believe at the same time that no one will deny that the heat of the earth increases as the centre is approached, and that the temperature of the interior may be very great. The crust of the earth was at one time supposed to be only 25 to 30 miles thick, because the increase of heat at that depth would be sufficient to melt any of the substances we are acquainted with on the surface —repetition again; but for many years past it has been deemed necessary to increase the thickness to even hundreds of miles, for reasons some of which will be alluded to in due time; and if, even at these depths, the increase of heat were only sufficient to fuse all the substances we know, it is very certain that at the interior surface of the shell it must be very much greater, as heat from there could only be *conducted* outwards, and the difference required to cause conduction, of any considerable degree of activity, through more than 2000 miles must be enormous, according to the experiments made by various physicists upon metals, which have a very much higher conducting power than rocks, and especially strata, of any kind. Therefore there can be no doubt, we think, that the inner surface of the shell must be at a very much higher temperature than what would preserve it in its liquid state, and that the matter composing it is liquid to a depth where it might be solidified by the pressure of superincumbent matter. We do not see how convection currents could be instituted, much less kept up, in melted matter, under the viscosity, and, at least quasi-solidity, sure to be produced by pressure of tens of thousands of pounds on the square inch, and therefore we do not take them into account. Any way, whatever may be the temperature of the interior surface of the shell, the same must be that of the imprisoned gases, because there convection currents could and must exist—were they even only created by the rotation of the earth and attraction of the moon—and cannot fail to keep the whole of the hollow part at the same temperature. It would be absurd to suppose that these gases could be at a lower temperature than the upper layers, counted from the region of greatest density, of the interior surface of the shell.

This section of our work may now be brought to a close by stating the conclusions at which we have arrived, leaving the results involved by them to be discussed separately, which we shall proceed to do immediately without binding ourselves so strictly, as we have done hitherto, to the avoidance of anything that may be looked upon as theorising or speculating. We believe we have conducted our operations in the most strict conformity to the law of attraction, and have no doubts whatever about the form of the interior of the earth resulting from them ; but there may be some room for small variations in the details of the various densities, and the position of the interior surface of the shell, arising from the pressure of the gases in the hollow centre, and the weight they will, in consequence, add to the general mass of the earth. The conclusions are as follows :—

(1) That the earth is not solid to the centre, nor is it possible that it could be, according to the law of attraction, but is a hollow sphere.

(2) That its greatest density must be at the region where the greatest mass of matter is to be found—as must have been always the case from the time it was a globe revolving on its axis, whether gasiform, liquid, or solid—which is now at 817 miles deep from the surface; and that the greatest density may not be much more than the mean of 5·66 times that of water ascribed to it by astronomers.

(3) That the inner surface of the shell of the hollow globe cannot be much over or under 2000 to 2200 miles from the outer surface.

(4) That the hollow part of the globe must be filled by an atmosphere consisting possibly in part of vapours of the chemical elements, and by gases at a very high degree of pressure.

(5) That the region of greatest density, and the position of the interior surface of the shell, may be expressed with very approximate accuracy as follows :—The former must be at 0·7939 of the mean radius of the earth, and the latter at 0·5479 of the same; both counted from the centre.

(6) That if the earth is a hollow sphere, the same must be the case with all the major planets and their satellites, the sun, and all the suns, or stars, that are seen in the heavens ; and that their interior proportions and form must be in much the same ratios to their radii as those we have found for the earth.

CHAPTER XI.

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CONSEQUENCES OF THE EARTH AND MOON BEING HOLLOW BODIES.

The Earth.—The idea that bodies such as those of the solar system, even of the whole universe, have their greatest density where the greatest mass is and are hollow spheres, is so natural and logical, more especially if it is supposed that they have all been formed out of some kind of nebulæ, that it seems strange it has never been brought forward prominently before. We say prominently because we know that the earth has been considered to be a hollow sphere by very eminent men, such as Kepler, Halley, Sir John Leslie, and by others of less name long after them. In support of this last remark, we shall make a few extracts—with comment on them—from an article on the "Interior of the Earth" in "Chambers's Journal" for February 1882, which have some interest in connection with our work.

1. "The great astronomer Kepler, for instance, in seeking to account for the ebb and flow of the ocean tides, depicted the earth as a living monster, the *earth animal*, whose whale-like mode of breathing occasioned the rise and fall of the ocean in recurring periods of sleeping and waking, dependent on solar time. He even, in his flights of fancy, attributed to the earth animal the possession of a soul having the faculties of memory and imagination."

If it could be believed that Kepler had any idea of the earth being formed out of a nebula, whether hollow, or solid to the centre, the idea of a breathing animal was almost a consequence, because the attraction—a thing he is supposed to have known nothing about—of the original nebula for the earth one, on matter so light as nebulous matter, would raise enormous tides and make the earth, in its then state, not far from like an enormous primitive bellows made out of goat-skins. No one knows what dreams may have passed through his brain. The last part of his notion was altogether fanciful.

2. "Halley was opposed to the idea of the globe being solid, 'regarding it as more worthy of the Creator that the earth, like a house of several storeys, should be inhabited both without and within.' For light, too, in the hollow sphere, he thought provision might in some measure be contrived." This notion appears to be altogether fanciful, the fruit of an enthusiastic, exuberant imagination, leaving no trace of scientific thought upon the subject.

3. "Sir John Leslie, like Halley, conceived the nucleus of the world to be a hollow sphere, but thought it filled, not with inhabitants, but with an assumed 'imponderable matter having an enormous force of expansion.'" It would be interesting to know on what bases he formed his ideas, as the filling of the hollow with imponderable matter seems to show more method than the former cases, but we have never seen

any allusion made to his theory anywhere, except in the article we are quoting from. There may have been some reasons given for such a supposition in his "Natural Philosophy," but when we began to read that work in times long past, a more modern one was recommended to us, and we lost the chance, never to return.

There are other theories referred to in the article, but we shall take notice of one more only.

4. "A certain Captain Symmes, who lived in the present century, was strongly convinced of the truth of Leslie's theory. He held that near the North Pole, whence the polar light emanates, was an enormous opening, though which a descent might be made into the hollow sphere, and sent frequent and pressing invitations to A. von Humboldt and Sir Humphrey Davy to undertake this subterranean expedition! But these imaginative conceptions must one and all be set aside, and the subject treated on more prosaic, though not less interesting, lines."

This conception of Captain Symmes will probably be looked upon as the most absurd of the whole lot, but to us it seems to give evidence of more thought than any one of them. One would think that he must have formed some notion of how a hollow sphere, with an opening out to the surface at each one of its two poles, could be formed. We must note that he lived in, possibly after, the time of Laplace.

We doubt whether anyone has ever studied out thoroughly how even a solid sphere could be ultimately elaborated from a nebula. It has always been a very general idea that a condensing and contracting nebula would, under the areolar law, assume the form of a lens rather than of a sphere. If this be so in reality, we may ask: How can the law of attraction produce a sphere out of a lens-shaped mass of rotating vaporous or liquid matter? It seems evident that to bring about such a result attraction must cease to act altogether in the polar directions, and only continue to draw in the matter from the equatorial directions of the lens, till the desired sphere was formed; and, How were the action and inaction of the law of attraction to be regulated meanwhile? Or, when the time came that a sphere of a pre-arranged diameter could be formed, a goodly part of the lens must have been cut off and abandoned; in which case we have again to ask: What was done with the surplus, the cuttings? No doubt they could be used up in meteor swarms, comets, or something; but Captain Symmes's theory has opened up a field for a good deal of thought, and our present knowledge of polar matters prevents us from being sure that strange discoveries may not be made as to the condition of the earth at the poles, although there may not actually be holes into the hollow interior. With regard to the last sentence of the quotation, we fully agree and are doing our best to comply with it. And in so doing, we shall have to return to the formation of globes out of nebulæ, elaborated into something more advanced than even lens-shaped discs.

There is no doubt that the reasons assigned by most, if not all, of the authors of the notions above cited are very fanciful, but one can hardly believe that the true reason—why the earth must be hollow—has not occurred to some of them; and that they did not follow it out because it involved too much work, and they did not feel inclined to undertake it, or had not time. On the other hand, modern astronomers and physicists have been so fascinated by the discoveries they have made, and in following them up, that the temptation to go on in the same course has been too great to allow them to spend time on the investigation of sublunary and subterranean affairs. Some of them have indeed studied the interior of the earth for special purposes, such as the thickness of the crust, solidity or liquidity, stability, precession of the equinoxes, the action of volcanoes, etc., etc.; but they never, apparently, examined into any of these features to the very end, otherwise, we believe, they would have come long ago to the same conclusion as we have. And withal it seems wonderful how near some of them have come to it. To most people it would appear absurd to think that any part of the earth of any great magnitude can be hollow, if in order to make up its mass its average specific gravity must be 5·66—more especially, if we tell them that the greatest specific gravity at any place need hardly exceed 5·66—forgetting that weight or mass can be taken from the interior where the volume per mile in diameter is small, and be distributed near the exterior where the volume per mile in diameter is comparatively immensely greater. But in whatever light we look upon the conclusions we have arrived at, a change in the construction of the bodies in space from solid to hollow spheres must produce changes in our ideas of them, and have consequences of great importance, too numerous to be all taken account of; we shall, therefore, only take notice of the most prominent.

Looking at the earth as a hollow sphere, we get rid of the difficulty of conceiving that matter can be compressed to three or four times less than the volume it has as known to us; and also of the misplacement of metals to the incredible degree we have shown to be necessary to make up its whole mass according to the sorting-out theory. And if we can only be bold enough to look upon gases as ponderable matter that can be compressed to great density, and so added to the weight of the whole mass, we may not be under the necessity of compressing the known matter composing it to even the half of its volume.

Somewhere in the first quarter of this century (see "Edinburgh Review," January 1870) Mr. Hopkins argued that the solid crust of the earth must be at least 800 to 1000 miles thick, in order to account for the precession of the equinoxes and nutation, but about a quarter of a century afterwards M. Delaunay demonstrated before the French Academy by actual experiment that the thickness of the crust had no bearing whatever on the problem. And about the same time Lord Kelvin inferred from the same thickness of crust that "no continuous liquid vesicle at all approaching to the dimensions of a spheroid 6000 miles in diameter could possibly exist in the earth's interior without rendering the phenomena of precession and nutation sensibly different from what they are"; and that the earth, as a whole, must be far more rigid than glass and probably more rigid than steel, "while the interior must be on the whole more rigid, probably many times more rigid, than the upper crust." With the theory of a hollow shell, a better foundation is given for Mr. Hopkins's argument than a solid crust at about the same depth as he assumed, while at the same time the liquid vesicle of 6000 miles in diameter is removed, which Lord Kelvin showed would change the phenomena of precession and nutation. We have seen that imprisoned gases may have a high degree of density, and consequently rigidity, and may in some measure supply what was required by Lord Kelvin, who knows, also, very well that a structure with some degree of elasticity in it is stronger than one that is absolutely rigid. Moreover, the shell of the earth, composed of solid materials at a very high temperature, and consequently so far plastic, could not fail to accommodate itself to any variation of centrifugal force that could take place. Variations in rotation of the earth could only have come on extremely slowly, and even the most rigid matter we know will gradually yield to extreme pressure long continued. But this subject of the plasticity of the most solid part of the interior was discussed and, it may be said, demonstrated during the meeting of the British Association of 1886, as reported in "Nature" from July to September of that year. Any way, the possibility of plasticity is most patently shown by the hollow-sphere construction of the earth.

We do not know what were M. Delaunay's proofs that the thickness of the crust has no bearing whatever on precession and nutation, but if they were complicated with the fluidity, or even viscosity, of a liquid interior beyond a depth of 800 to 1000 miles, they must be entirely changed under the notion of a hollow sphere where there could be no really liquid molten matter, except near the inner surface. One thing we may be certain of, and that is, there must be something to account for precession and nutation, and we believe that the hollow shell, with the greatest density where the mass is greatest, is a much more rational cause for these phenomena than the bulging out of the earth to the extent of 13 miles or so at the equator.

It is very difficult to find out what geologists consider to be the nature of the interior of the earth in its details, but for our purpose no particular knowledge is required. However, it is necessary to allude to the principal features of their theories in order to note and remark how far they will agree to, or be facilitated, or the reverse, when applied to a hollow sphere. It would seem that almost all geologists are agreed that the central part is solid, and possibly extremely rigid owing to the enormous pressure of superincumbent matter; that it has a solid crust of several hundreds of miles in thickness; and that under this there is a sub-crust divided into two or more layers of different densities, partially liquid or at all events plastic, extending all over the solid interior matter; the chief purpose for which it is required being apparently to supply matter for volcanic action and surface movements.

Under the theory we are advocating, the place of greatest density of the interior is calculated to be at 817 miles from the surface, and its greatest approach to solidity will be there also; consequently, if geologists consider that it will have sufficient plasticity there to provide matter for volcanic eruptions, they will be at one with us so far. But should they consider that they require, for volcanoes, matter more liquid than is likely to be found at that depth, they will have to place their magma layers either much deeper or somewhere between that depth and the surface, in which case they will encroach on the requirements of astronomers, without liberating themselves from a difficulty in which they must find themselves involved under their present ideas. They say that these plastic layers exist under the solid crust all round the interior of the earth, so that if one of the duties they have to perform is to keep the various chains of volcanoes in communication with each other, their lateral movements must extend to some hundreds of miles in the cases of the enormous volumes of matter that are sometimes thrown out in even modern eruptions, and they have to provide the means for procuring that lateral motion. Shrinkage from cooling, or falling in of part of the solid crust, might bring about these enormous outbursts of lava, but they would be more likely to produce simple overflows than the explosive ejection of such masses as are now being recorded from time to time. We have brought into remembrance, page 148, that water cannot penetrate into the interior of the earth to a greater depth than 9 miles, more or less, as water, and that beyond that depth it can only exist in the form of steam, or dissociated into its elements of hydrogen and oxygen. As long as it continued in the form of water it could be suddenly flashed into steam, of not far from two thousand times its volume, by relief from pressure or sudden application of heat, and thus be converted into a violent explosive almost instantaneously; but when it came to have the form of a gas, it could only be heated gradually the same as any other gas. It is clear, therefore, that water cannot be looked to for producing the force, explosive or otherwise, that is required to raise even molten matter from depths of hundreds of miles to overflow from the summits or outlets of volcanoes.

A pressure of 400 atmospheres would be required to balance a column of average rock of *one mile* high. A mass of water, through shrinkage of the crust, might get introduced to the vent of a volcano, or some cavity connected with it, a few miles under the surface of the earth and cause an earthquake—it might be introduced by an earthquake—or eruption or both, abundantly formidable and destructive, no doubt, but only comparatively superficial, such as those of Naples and Charleston, where the extreme depth was calculated to be only a few miles; but it seems to us to be totally inadequate to produce those outpours that last for days and weeks, covering leagues of land, and filling up bays of the sea, with floods of lavas. It may be the principal agent or ally in producing the horrors and devastation of a grand eruption that has invaded the regions of water, but it is not to be conceived as possible that it can be the prime cause. The volumes of steam, water, and mud thrown out on such occasions, only tend to distract our attention from looking deeper for the true cause of the eruption. Geologists are therefore thrown back upon their magma layers to look for the motive power for producing these grand eruptions, and they cannot get water down deep enough to do it.

Tides produced by the sun and moon cannot be appealed to, otherwise the eruptions would be more or less uniform in their periods of occurrence. Sudden evolution of gases in the magma layers could not be accounted for in any way known to us, and accumulation of gases would involve the idea of immense cavities, to serve as reservoirs to be gradually filled till the pressure was sufficient to force a way out, and would imply a formation of the interior in compartments specially adapted for particular purposes, and altogether too fanciful to be entertained. Where could such enormous masses of matter, as those thrown out, come from at only a few miles from the surface? The great eruption at the Sandwich Islands, of about a century ago, after flowing over a distance of many miles of land, on which it left enormous quantities of lava, filled up a bay of the sea twenty miles long, and ran out a promontory of three or four miles into the sea; and we cannot conceive it to be possible that such a quantity of matter could be blown out from something less than 9 miles deep by water suddenly flashed into steam.

The critical temperature of water—that temperature at which it changes into steam under any pressure however great—being 412°, its pressure in the state of steam will be somewhere about 7150 lb. per square inch, let us say 500 atmospheres; then, if 400 atmospheres are required to balance 1 mile in depth of average rock, as we have stated above, the pressure of steam just cited would balance only $1\frac{1}{4}$ miles of rock. We can, therefore, see how inadequate it would be to force a column of lava up from even the depth of 9 miles. At that depth 3600 atmospheres of pressure are required to balance a column of lava, and there are only 500 available. It has been said that the downward pressure of steam would force up the lava through the vent of a volcano, but an arrangement of that kind would require a downcast shaft as well as the upcast one of the vent like as there are in collieries; but the downcast would have to go very deep to compress the steam—a gas now—to the required number of atmospheres. Far more likely that the steam itself would put an end to any increase of water, by driving it back through the channels by which it was descending; for if they are supposed to exist under a solid crust of 800 miles thick the pressure required would be 320,000 atmospheres, and with a crust of only 100 miles thick 40,000 would be required. The only way, therefore, in which volcanic eruptions can be produced in the earth, if solid or liquid, or partially solid and partially liquid, to the centre—in other words, from magma layers—is by the shrinking of the crust squeezing out the lava. With a hollow earth and shell of more or less 2200 miles in thickness, liquid to some depth on the interior surface the difficulty becomes very much less. The communication between the vents of volcanoes would be complete and simple, without any lateral forcing of the lava through magma layers made expressly for the purpose; it would be an open and natural flow from one place to another. That there are such volcano vents connected with each other has been very generally believed, and even almost proved by observation of eruptions taking place in two or more almost simultaneously, or at the least showing signs of violent agitation, the motive forces for which would be the gases which we have concluded must be imprisoned in the hollow centre. When their pressure came to be sufficient to blow or force out the liquid, or semiliquid matter, bubbling and boiling in the vents in constant activity, there would be an eruption, during and after which the gases would escape till their pressure was greatly reduced, when the volcanoes would return to their semi-active state. The gases would naturally be those of the many kinds that are found in eruptions, by reason of their being generated in the earth, mixed with steam and water in the manner we have already shown.

Let it not be supposed that the gases would require to have force enough to raise lavas from depths of over 2000 miles from the surface. According to our arguments for a hollow earth, at 817 miles from the surface the two halves —outer and inner—of the matter composing it meet and balance each other, so that all the pressure required would be what is necessary to overcome the inertia, viscosity, or cohesion of the matter in the vents. What that would be we do not pretend to be able to calculate, but we believe that it would be very much inferior to that required to balance a column of lava of even 100 miles high. We have seen that gas compressed to 4835 atmospheres would be $6\frac{1}{4}$ times more dense than water, and of equal specific gravity to the heaviest matter required in any part of the earth to make up its average density to 5·66 that of water, and we cannot assume any greater pressure than this, without diminishing that maximum. If that, or any lesser degree of compression, would supply the necessary force, then all difficulty is removed further than pointing out the means of keeping the volcano vents open or openable; and the quality of openable may be facilitated by the contraction of the interior from cooling. If a greater pressure be necessary, we need not be afraid of greatly increasing it, for the only consequence would be to diminish the maximum density of solid matter required in any part of the earth, to make up the general average to 5·66, which means less compression of the matter. If the idea of the accumulation of gases in the hollow centre, or of the hollow centre itself, is inadmissible, then scientists in general can continue as before with their magma layers—aqueo-igneous if they like—but they must abandon the notion of lavas being expelled from them by steam pressure. We repeat that steam could never get down in the form of steam to the depths they require. The temperature there would be more than sufficient to resolve it into its elements of oxygen and hydrogen, and it would behave very much like the gases we have supposed to be in the hollow; there might be accumulation, but there could be no sudden flashing into existence like steam from water.

In support of our observation—if it needs support—that water as water cannot penetrate into the earth to a greater depth than where it meets a temperature of 412°, we may refer to reports on earthquakes of comparatively recent occurrence. We learn from the "London Quarterly Review" of January 1869, that in the Neapolitan earthquake of 1857, Mr. Mallett found the greatest focal depth to have been $8\frac{1}{8}$ geographical, or 9·35 statute miles, which agrees very well with the depth to which water could penetrate and be suddenly flashed into steam. (We say nothing, for the present at least, about how the water and the heat managed to meet so instantaneously.) The shock of the instantaneous generation of steam might be felt much lower, but it would tend to interrupt, not to produce, the eruption of lavas. In speaking of the pressure on the walls of the cavity, where the shock was produced, being 640,528 millions of tons, the reviewer says, "it may have been greater because the steam might be supposed to have acquired the temperature of the lava," and that is 2000° F.; but that could not well be. In order to meet lava of that temperature the steam would have to descend to from 20 to 25 miles deep; on the other hand, if the lava is assumed to have entered the cavity, it could only do so at a comparatively low velocity and would not reach more than a fraction of the steam at a time, and even for that reason there could be no flashing, as steam is only a gas, and cannot be heated otherwise than as a gas. Here the spirit of facilitating the meeting of the lava and the steam, is as apparent as in bringing about the meeting of the water and the lava noticed above. On the whole, therefore, we think that we were right in saying that steam or water cannot be the cause of volcanic eruptions, but that the invasion of the domains of water by the lavas may be the cause, in the main, of the explosive part of eruptions, and of the most disastrous effects of earthquakes. Moreover, the focus of the Neapolitan earthquake was 75 miles distant from Vesuvius, and therefore far removed from anything like direct connection with the vent of the volcano, so that water from it in any form could have no effect upon the magmas of scientists.

"The Scientific American" of July 16, 1887, tells us that Captain E. D. Dalton has calculated that the depth of the Charleston earthquake was 12 miles; statute miles, it is to be supposed, as nothing is said to the contrary. To reach the temperature of 412° this would give an increase of 1° in 45 metres in depth, which is a considerably greater depth than what we have estimated, but does not invalidate our reasoning, as it has always been known that the gradient of increase of heat varies considerably from one place to another. Besides, and more especially, Charleston being a seaport, and, consequently, not far from the level of the sea, it is to be supposed that, owing to the presence of water, the cooling of the earth has penetrated to a greater depth there than in the heart of Italy. The same authority states that in the formidable Yokohama earthquake of 1880, the mean depth was only $3\frac{1}{4}$ miles. The mention of mean depth here makes us notice that the 12 miles may have been the extreme depth to which the earthquake, or shock, was felt at Charleston, and that the focal depth may have been considerably higher up than that. Be that as it may, there is no proof existing that water or even steam can penetrate into the earth more than a very few miles, much less to hundreds of miles.

Having referred pretty freely to the aqueo-igneous magmas, supposed by some scientists to exist deep down in the interior of the earth, it is but fair to give our reasons for refusing to believe that there can be any such mixture in any part of it, or anywhere else. In order to do so, we shall first cite some of the bases upon which such ideas have been founded. In "Nature" of December 12, 1889, we find what follows :—

"Let us now consider the alternative theory suggested by Mr. Fisher. He claims that geologists furnish him with a certain amount of positive evidence for the idea that water is an essential constituent of the liquid magma from which the igneous rocks have been derived. Passing over the proofs of the existence of water in the crystals of volcanic rocks, and in the materials of deep-seated dykes, let us come at once to the granite, a rock which can only have been formed at great depths and under great pressures, and which often forms large tracts that are supposed to have been subterranean lakes or cisterns of liquid matter in direct communication with still deeper reservoirs. Now, all granites contain crystals of quartz, and these crystals include numerous minute cavities which contain water and other liquids; and the quartz of some granites is so full of water-vesicles that Mr. Clifton Ward has said : 'A thousand millions might easily be contained within a cubic inch of quartz, and sometimes the contained water must make up at least 5 per cent. of the whole volume of the containing quartz.' This amount only represents the water that has been as it were, accidentally shut up in the granite, for some was doubtlessly given off in the form of steam which made its way through the surrounding rocks."

We cannot follow Mr. Fisher in "passing over the proofs of the existence of water in the crystals of volcanic rocks and in the materials of deep-seated dykes"; because the presence of water in these crystals when examined in a laboratory is no proof that water was present in them when they were liquid, and before they put on the form of crystals. There is no analogy between them and General Wade's road. Any crystals that a man can pick up anywhere, even from the mouth of a volcano, are quite capable of absorbing vapour of water from the atmosphere before he can carry them to his laboratory. All matter is supposed to be pervaded, more or less, by the ether, and there is always an open road for it, i.e. the vapour of water to enter by. Nature dives more rapidly into a piece of rock than a man can walk or drive down from the summit of a volcano, so that getting water out of it when he is in his laboratory, is no proof that the water was there when the piece of rock was at the bottom, not the mouth, of the volcano. The minute so-called water-vesicles in granite have only served the purpose of a snare to facilitate his deceiving himself, by the help of Mr. Clifton Ward, to further his speculations. For we think it would have been far more natural for him to have supposed that these vesicles were originally filled with the all-pervading ether. Or, are we to prohibit the ether from being present anywhere, except where it suits us? Even the dimensions given to the vesicles of a thousand millions of them being contained in a cubic inch makes us at once think of something more ethereal than water. And the whole object of Mr. Fisher's argument is to show how the depth of the ocean may be increased by water expelled from such magmas.

A hollow planet, with compressed gases in the centre, raises the idea of the possibility of explosion. It would have furnished Olbers, or any follower of his, with the bursting force to shatter into fragments the planet, out of which he supposed the asteroids to have been made. It need not cause any alarm with respect to the earth, whose shell is very much thicker than that of the exploded planet, seeing that its whole mass has been estimated not to have exceeded one-fourth of that of the earth (see Table I.). The 5000 atmospheres of pressure we have spoken of could have no such effect on so thick a shell as the earth's; and we cannot increase the number without diminishing its average density, as we have shown. When we see Mars blown up, whose diameter, and consequent thickness of shell, are not much more than half those of the earth, we may begin to think of getting out of the way.

THE MOON.

This satellite is supposed, according to the nebular hypothesis, to have been at one time neither more nor less than a smaller edition of the earth itself, endowed with atmosphere, plains, mountains, volcanoes, rivers, seas, rotary motion, etc.; previous to which it had passed through the same stages of gasiform, molten-liquid, and solid as its parent had done. One would think that its almost perfectly round form proves to demonstration that it must have rotated rapidly on its, or an, axis at one time; but there are some astronomers who think that it has never rotated at all, an opinion in which we cannot concur by any means. When it arrived at the stage of having seas, the tides raised in them by the attraction of the earth must have acted like a brake on its rotation—in the same manner as its attraction is supposed to be now doing on the earth—and gradually reduced it until it ceased altogether; from which time forward it must have always presented the same side to the earth. It has been thought that the tides raised in it by the earth would be so tremendous that they would prevent anything like rotation having ever existed; but everything requires to be accounted for, and the only way to account for its perfectly circular form is by its having rotated.

Considering, then, the moon as having been dispossessed, absolutely, of rotation and reduced to the single motion of revolution round the earth—as far as we are at present concerned, at least—we can go back to the period when this change came over it, and consider what would happen about the time, and immediately after the rotation came to an end.

When a fly-wheel is made to revolve rapidly and is then allowed to run until it stops, it very seldom comes to rest all at once, and generally swings backwards and forwards something like a pendulum, until it finally stops; because it is always a little heavier on one side than the opposite, even should the difference of weight be only that of the handle by which it was set in motion; so we may suppose it would be with the moon when at last it failed to turn the centre, as it is called—the tides, the retarding cause, giving origin to the difference of weight on opposite sides—and we can conceive what commotions would be created on its surface by the wobbles it would make. We can imagine how the seas would rush backwards and forwards over the lower land and hills, levelling them down to the flat plains that are seen spread abroad among the innumerable volcanoes which cover the side turned towards the earth, until it finally came to rest. When the commotions ceased and the centrifugal force of the moon's revolutionary motion round the earth—which is over 38 miles per minute—came to act freely, we know that the atmosphere and seas, being the mobile parts of it, would be pretty nearly all driven off very quickly to the side farthest from the earth, perhaps even before it came to the final state of comparative rest, whose translation would involve mighty rushings of waters there as well. Also, that all the liquid matter in its interior, being so much heavier and more difficult to be moved by centrifugal force, would gravitate towards the side nearest the earth, whose attractive force would soon put an end to anything in the form of interior tides of molten matter, which very probably existed up till that period. If the moon came to a stop without any wobbling, then the transference of atmosphere and seas to the farthest off hemisphere, and the gravitation of the liquid matter of the interior to the side nearest to us, might be more gradual but would finally and certainly come to pass. And here we must specially note that if it made one rotation for each revolution, or one rotation in any length of time or under any circumstances whatever, these transferences of matter from one hemisphere to the other could not have taken place, because there would be no stationary region to which they could be transferred by centrifugal force, as each part of its circumference would in its turn occupy that region. And above all—be it specially marked—because the moon would not, in that case, always present its same side to the earth.

Looking upon the moon as a hollow sphere of somewhat the same proportions as we have made out for the earth, the region of greatest density would be at about 234 miles deep from the outer surface, the interior surface of the shell at the depth of 692 miles, and the hollow centre 776 miles in diameter, as long as it continued to rotate upon its axis. When that motion ceased and the seas were transferred to the hemisphere farthest off from the earth, and the liquid matter in the interior had gravitated towards the nearest, as we have just said above, its conditions would be very materially altered. Lest it should be supposed that with a very thin crust, nearly its whole mass would gravitate to the side nearest to the earth, let us always bear in mind that the moon would be virtually solid to not far from the inner surface of the shell, through the pressure of superincumbent matter, both from without and from within, in the same manner as we have considered the earth to be. Whatever water had been absorbed by the crust when it was still rotating on its axis—which, at most, could have penetrated only a few miles—and even whatever lakes or inland seas might have been left on the surface always seen by us, would be soon evaporated by the internal heat, and the heat radiated by the sun—which Sir John Herschel has calculated to be greater than boiling water—and driven off in the shape of vapour in the same manner as the atmosphere had been. These transferences would lead to two consequences, each one of its own nature, which we must not fail to notice particularly, as in great measure they explain to us the constitution, or rather the construction, of the moon. (1) All air and vaporous matter being translated to the unseen hemisphere would tend to cool it more rapidly and deeply than the other, not only on account of the cooling powers of the water, but from the atmosphere and vapours preventing the heat of the sun from acting so powerfully upon it. (2) On the other hand, owing to the accumulation of melted, or liquid, matter in the interior of the side now turned permanently towards the earth, the formerly solid part of that side would tend to increase in temperature, which, joined to the heat from the sun not intercepted by any atmosphere, and continuing without interruption for a fortnight at a time, would produce a great difference in the temperatures of the two hemispheres. Thus it is natural to suppose that the thicker and cooler solid shell on the one side would tend to weaken and drive down the volcanic forces to a greater depth; while the greater temperature and thinner solid shell on the other, the down side—the one next to the earth—would have an exactly opposite tendency and would bring them nearer to the surface. In this manner we seem to find a very plausible reason for the great exuberance of the volcanic forces displayed on the surface of the moon always presented to us.

Both the interior construction and exterior form of the moon, as modified by losing its rotary motion, would no doubt be very different to that of a hollow sphere rotating on its axis; but Hansen's "curious theory" has prepared us for this, by showing that some anomaly in its construction had been noted and commented upon, although the existence of the anomaly was not attributed to the atmosphere on its having been driven away to the far-off hemisphere. But with this subject we have dealt pretty fully already in Chapter II., which may be referred to for further explanation if required.

CHAPTER XII.

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215 Some of the results arising from the sun's being a hollow sphere.
216 Repetition of the effects of condensation on the temperature of the nebula.
217 Ideas called up by the apparently anomalous increase of temperature.
218 How heat is carried from the sun to the earth.
219 The sun supposed to radiate heat only to bodies that can receive and hold it, and not to all space. The heat of the sun accumulated in a hot box to considerably beyond the boiling point of water.
220 The heat accumulated in this way supposed to be due to a peculiar function of the ether, as it is a fact that heat can be radiated from a cold to a hot body.
221 The sun must be gaseous, or rather gasiform, throughout. No matter in it solid or even liquid. Divisions and densities of shell.
222 The hollow centre filled with gases, whose mass naturally diminishes the mean density of the whole body.
223 The amount of this reduction so far defined. The presence of gases or vapours in the hollow a natural result of condensation.
223 The hollow centre filled with gases not incompatible with the sun's being a hollow sphere. The temperature at the centre may be anything, not depending on any law of gases.
224 Further exposition of hollow-sphere theory put oft till after further development of the construction of the sun.

In the last chapter we have endeavoured to point out how much our knowledge of the interior construction of the earth and moon has been increased, and how many difficulties in the comprehension of their construction are overcome by the fact demonstrated in previous parts of our work that they are hollow bodies ; and we now proceed to show some part of what may be learned from studying the sun under the same conception of its being a hollow body. We say part of what may be learned, because the whole seems to us to be so great that it would take much more time and space, not to speak of knowledge, than we can devote to the subject to make even a proper beginning to such a study. To our sight it takes away the necessity for guessing in the dark at what the construction may be, which is all that has hitherto been done; and furnishes the means of discovering, with intelligent study and investigation, what most probably is the actual constitution of the sun.

In Chapters V. and VII. we have followed up the contraction and condensation of the residue of the original nebula, after it had thrown off all the known planets; first, to the diameter of 58,000,000 miles, with density of $\frac{1}{274}$th of an atmosphere and temperature of $-273°$, or *one degree* of absolute temperature; second, to about 9,000,000 miles diameter, with density equal to air at atmospheric pressure, and temperature represented by zero of the centigrade scale, or what has been hitherto called 274° of absolute temperature; third, to 4,150,000 miles diameter, with density equal to ten atmospheres and temperature of 2740° of actual, or 2742° of absolute temperature; and fourth, to 972,895 miles diameter, with density equal to water and temperature which we do not venture to express. All these stated densities and temperatures are understood to be average, the temperatures being those the various stages would have had, had no heat been radiated into space by them.

Here, then, we might go on to set forth what might be the interior dimensions, various densities, and conditions of each one of the four stages, under the conception of their being all hollow spheres, and afterwards carry on a *résumé* of the whole of them and apply it to the sun as it is at the present day; but this, in addition to involving an immense deal of difficult work, subject to errors and omissions in operation, would not do much towards enabling us to explain in a more simple way what may be, most probably is, its interior construction. We shall, therefore, look upon the four stages as represented by a model having the diameter and other known measurements of the sun in its present state.

To begin what we propose to do we believe it is necessary to repeat, as a thing that has to be borne in mind, that when we had contracted the original nebula from 6,000,000,000 miles in diameter to 58,000,000 miles, its density was only equal to a barometric pressure of *one-ninth* of an inch of mercury, and its mean temperature had been increased only *one degree*, that is, from $-274°$ to $-273°$; and we can add that, although we had given the original nebula ten times that diameter, the result both in density and temperature would have been the same when it was condensed to 58,000,000 miles in diameter. Then, again, we believe it necessary to repeat that by contracting the nebula from 58,000,000 to 9,000,000 miles in diameter its mean density was raised from $\frac{1}{274}$th of to full atmospheric pressure, and its mean temperature from $-273°$ to zero of the ordinary centigrade scale, i.e. to the temperature of freezing water. These two results strike us, at first sight, as somewhat remarkable, seeing that what looks like almost unlimited condensation to 58,000,000 miles diameter produced only one degree of temperature, while the comparatively insignificant condensation of from 58,000,000 to 9,000,000 miles in diameter produced 273° of heat, *in the way we are accustomed to measure heat.*

Following up these two facts gives rise to ideas that have been borne in upon us ever since we stumbled upon them when making the analysis of the nebular hypothesis. One of these notions was that, were it practicable, the most effectual mode of liquefying gases would be by putting any one of them into a sealed vessel, and confining it in another vessel in which a vacuum of $\frac{1}{274}$th part of an atmosphere could be produced; no difficult matter as far as the vacuum is concerned, for a good exhausting air-pump would be all that is required. But the practicability? The vessel in which the vacuum is produced would have to be protected so that no extraneous heat could be conveyed or conducted into it in any way whatever. How this could be, or is, done without cutting off every possibility of manipulating the enclosed vessel, we do not see; but it seems evident that some method is available because something presenting the same difficulties has been actually done, as everybody knows. The only degree of vacuum of any use in the exterior vessel would be about one-ninth of an inch of mercury, because that would as we have just said, furnish a temperature of $-273°$. There would be no necessity for applying pressure to the gas experimented upon. In fact pressure would be an obstacle to the experiment, according to the theory of the air thermometer; and could only be of use by furnishing a larger quantity of liquid to be handled and examined.

Another idea is that there can be no such condition as absolute zero of temperature of what we are accustomed to think of as a gas, as far as science is concerned; as on arriving at that condition, perhaps long before, any gas would slip out of its hands altogether. But there is a much more rational reason than this, which we have brought forward on a former occasion. We are taught that heat is a mode of motion, which means that as long as there is heat there will be motion to account for it, so that motion would have to be annihilated on the earth before absolute zero of temperature could be reached. We have, then, to come back to what we said when treating of the heat of space, and look upon the temperature of the vibration of the ether as being the lowest that can be measured by science. We said then that it must be far below $-225°$. Since then a temperature has been reached of within 23° or 24° of absolute zero, according as that condition is measured by 273 or 274.

This, of course, leads us to think of the ether as a carrier of light, heat, etc., and of how it can carry heat to the earth without becoming heated itself, as there can be no doubt about its being a material substance. How it can bring what may be called considerable heat to the earth and still have little or no heat in itself; even should it turn out, which we do not believe possible, that the estimates of the heat of space of $-150°$ and $-142°$, made about the beginning of this century by Sir John Herschel and Pouillet, turn out to be near the truth. We have seen, in "Nature" of July 15, 1886, a monograph by Captain Ericsson, in which he shows that the heat radiated by the sun to where his rays strike our atmosphere is somewhere about 83° F., and it is not easy to see how radiated heat can be transmitted through 90 million miles of space at a temperature of much lower than $-225°$, and reach the confines of our atmosphere with the heat of 83° F. There is one supposition that occurs to us under which this can happen, and that is, that the sun only radiates heat to bodies which can receive it, and does not radiate it into all space where there is nothing but the ether to hold it. This, of course, implies that the ether acts the same part—the part for which it was really invented—with respect to heat that a telegraph wire does with respect to electricity ; in which case, we could imagine that it starts from the sun with the maximum heat radiated by him, and that this goes on decreasing in the ratio of the square of the distance it travels through, the same as is understood to be the case with all radiated heat ; and that the part of space not occupied, for the time necessary, by these connexions might be supposed to form the return current which we believe must exist, just the same as the earth does for electricity. For that there is a return current is demonstrated by the fact that the earth radiates heat into space when the sun is not shining upon it. Again, even in this case, we have another difficulty thrown upon us, over and above that cited by Captain Ericsson, of the heat delivered at the bounds of our atmosphere being about 83° F., by our being informed in "Engineering" of December 4, 1885, that: "A hot box, contrived to observe the temperature which could be attained by the unconcentrated solar rays, was used on Mount Whitney, 12,000 feet above the sea"—well within the limits of our atmosphere—"and that the enclosed thermometer rose to 233·3° F. on September 9, 1 p.m., 1881, the shade thermometer then reading 59·8 F." How are we to comprehend these two facts? We have seen a way of getting over part of the first fact as far as to the boundary of our atmosphere, but from there we have to carry 83° F. to the top of Mount Whitney, through the atmosphere there and present it along with the other lot in the hot box at 233·3 F.

We may get the beginning of what may be an explanation of all the facts from another part of Captain Ericsson's monograph, where he says: "Engineers of great experience in the application of heat for the production of motive power and other purposes deny that the temperature of a body can be increased by the application of heat of a lower degree than that of the body whose temperature we desire to augment. The soundness of their reasoning is apparently incontrovertible, yet the temperature of the mercury in the instrument just described raised to 600° F. by means of the parabolic reflector, increases at once when solar heat is admitted through the circular apertures, although the sun's radiant intensity at the time may not reach one-tenth of the stated temperature. It should be mentioned that the trial of this new pyrheliometer has not been concluded, owing to very unfavourable atmospheric conditions since its completion. For our present purpose the great fact established by the illustrated instrument is sufficient, namely, that the previous temperature of a body exposed to the sun's radiant heat is immaterial. The augmentation of temperature resulting from exposure to the sun, the pyrheliometer shows, depends upon the intensity of the sun's rays.

A little study shows us that the steam engineers are perfectly right in their doctrine. The heat of steam can only be called a variety of the temperature of water. At 300 lb. pressure per square inch the heat of steam is 417·5° F., while at 20 lb. pressure it is only 228·0° F., and therefore the steam engineer has good reason to say that steam at the lower pressure—or derived from heat that can only produce that pressure—can add no heat to the higher; on the contrary, the only possible means of applying the heat of the lower to that of the other would be by mixing them, and we know what the result of that would be. This brings before us the fact that the steam engineer's heat is very limited, and can only be communicated in certain ways, while the sun's heat is comparatively unlimited, and can only be communicated to anything through the medium of the ether. But it probably teaches more than that. Were the engineer's heat unlimited in quantity at low pressure it can easily be believed that it could be transmitted to another body at any temperature by radiation, the same as it is radiated from the sun to a hot box; but it is not, and we thus seem to find that radiation is a mode, possessed by the ether alone, of conveying heat from one body to another. It has nothing whatever to do with mixing, conduction, convection, or anything, except in so far as the ether is mixed in a more or less limited quantity with all matter. In support of this idea we can refer to Professor Tait's treatise on heat, where we find it stated that "heat does pass (though on an infinitesimal scale) from colder to hotter bodies"; and we can easily understand that the infinitesimal quantity so passed is due to the comparatively infinitesimal quantity of ether there is in either of the two bodies to perform the work of transference. Professor Tait has not told us how heat is carried from a cold to a hot body, but there can be no doubt about its being a function of the ether which can only be found out by a careful and analytical study of that agent. Such a study we propose to undertake presently without much expectation of being successful, but still with the hope of helping in some measure to find out how the ether operates. Meanwhile we shall return to what we had begun to say about the sun being a hollow sphere, and to our proposal to treat of the nebula contracted from 58 million miles to its present diameter, as if it were a model representing a *résumé* of all the effects produced on the nebula by that amount of condensation.

We know from all our work that the sun must be a gasiform body, which means that all the cosmic matter contained in it must be in the form of vapour, even although its consistence should outrival a London fog—notwithstanding that some physicists have supposed that it may be solid at the centre through extreme pressure—and it is not altogether correct to compare its construction to that of a solid body such as the earth; but as we have no other we shall begin to make a comparison with it, which, it will be found, can lead us into no appreciable error. Considering then the sun to be 867,000 miles in diameter, with mean density of 1·413 that of water, the hollow part being still completely empty, and applying to it the same proportion we have deduced for the earth, we find that the region of greatest density would be at 0·7937 of the radius of the sphere—a proportion really derived from the line of division into two equal parts of the volume of a sphere—from the centre, or 89,431 miles from the surface; and the inner surface of the shell at 0·548—a proportion derived from our calculations for the earth—of the radius of 433,500 miles, or 237,558 miles from the centre; which in turn makes the shell to be 195,942 miles thick, and the hollow centre to be 475,116 miles in diameter. On the other hand, still following the proportions derived from the earth, we find that the density at the surface might be one-third of the mean density or 0·471; that it might be one-fifth greater than the mean, or 1·7 at the region of greatest density and one-half, or 0·71 at the inner surface of the shell—all of these three densities being in terms of water.

Now, the hollow centre of 475,116 miles in diameter would have a volume of one-sixth of the whole volume of the sun, which, filled with gases, would diminish all these densities just in proportion to what may be considered the degree of compression and condensation the gases might be subjected to. That there should be gases in the interior hardly requires to be more than stated, as there can be no doubt that the degree of heat to which the shell had arrived by the time it came to have the dimensions above mentioned, would be amply sufficient to excite chemical action among the elements of which the sun is composed; and the gases or vapours produced by that action would flow as naturally towards the interior of the hollow centre as towards the space beyond the outer surface of the shell, until they were stopped by increase of pressure, which of course would mean increase of density in this case. We see then that if the hollow centre has a volume of one-sixth of the whole volume of the sun and we multiply this volume by 6, we have a mass equal to the whole mass of the sun, were its mean density only the same as that of water. Consequently, if we multiply the said volume by 6 and by 1·413, that is by 8·478, we get a mass equal to the whole mass of the sun at its known mean density. Again, were we to suppose the hollow centre to be filled with gases of the same specific gravity of air, condensed to a pressure of 6560 atmospheres—which would correspond in density to 8·478 times the density of water—we should have in the hollow centre alone a mass equal to another sun, in addition to the one made up by the dimensions and densities stated above. We see then that if we fill the hollow centre with gases at the pressure, and with the density just stated, we have a sun of twice the mass it should be. But if we leave the specified gases in the hollow with one-half of the above density, and deduct the equivalent mass of the other half density of the gases from the shell, as estimated for the hollow centre, we should have a sun of the mass required by astronomy. In this way we should have the three specified densities reduced from 0·471, 1·70 and 0·71 to 0·236, 0·85 and 0·355, for the outer surface, the region of greatest density, and the inner surface of the shell, respectively; and the pressure and density of the gases in the hollow centre reduced to 3280 atmospheres. Thus, from what has just been shown, which at first sight may be thought very irrelevant matter, we discover that it is not necessary that there should be any matter in the sun even so dense as water. And still we have to think of what an insignificant pressure three or four thousand atmospheres would be in the centre of the sun.

No one will pretend to allege that no gases can be produced in the shell of the sun, or to say anything against those formed in the inner half of it finding their way to the hollow centre, and going on increasing there till they were able to force their way out through the shell; that is, until their pressure was equal to the resistance offered by the gaseous body of the sun, or against their temperature increasing until it came to correspond to their density and most probably rising to a much higher degree. Such, then, must even now be the construction of the sun, as reduced to its present diameter and density. That is, a hollow sphere consisting of cosmic matter combined with gases and having a hollow centre filled with chemically formed gases or vapours.

Here it may be argued that the sun ceases to be a hollow sphere, but that is not so. The most that can be said about it is that it is a hollow sphere with the empty part filled up. It would only be in much the same condition as a hollow globe of iron filled with melted antimony or bismuth. Its construction would be in no way changed by the empty hollow being filled up, so long as its condition remained gaseous—not changed to liquid or solid. The only difference in our sphere would be that its density would virtually be the same from what we have called the region of greatest density to the centre, which would not only involve a greater distance of that region from the surface of the sphere, but another reduction of the above mentioned densities of the sun ; for we cannot in any way imagine that the pressure in its interior can be less than many thousands of atmospheres.

Whatever may be the relative densities of the shell and the gases in the hollow, they will have no necessary effect upon the temperature of the latter, because, let the densities be what they may, the gases might be cooled down to absolute zero of temperature, or raised to any imaginary degree without any change being made in their weight as long as their volume was maintained the same. This has been proved by laboratory experiments almost as far as possible. Gases at very high degrees of pressure and consequent densities have been cooled down to not far from the absolute zero of temperature, while others under very low pressures have been heated up to nearly as great heat as the enclosing vessel would bear, without their weight being altered in either case ; but in the sun there is a larger laboratory in which we can place no limit to pressure or temperature. We know, however, that pressures are required sufficiently great to blow out jet prominences with velocities of 100,000 miles per second or more, to heights 200,000 and even 350,000 miles above the photosphere ; and if we knew what these pressures are we might be able to learn something about the minimum temperatures of the gases. To obtain these pressures we have—in the construction we are advocating—a real containing receptacle with sides 195,942 miles thick, in the outer half of which we have the compressing force, due to the gravitation of the whole mass of the sun acting at the centre, and over and above, both in it and the inner half, we have the cohesive force of the matter of which it is composed. In fact we have a sun whose construction we can understand, in which we have gases shut up without their expansive forces being impaired in any way, ready to be exerted with full energy whenever they are relieved from compression by any commotions in any part of the whole body, and taking their part in keeping the whole of the matter composing it in constant motion. How these commotions are produced it is not difficult to explain to a very considerable extent at least, but this we must leave over until we have reconstructed the original nebula, and shown how the solar system could be elaborated from it, almost exactly in the way conceived by Laplace in his nebular hypothesis. We shall then also be able to extend our exposition of what is to be learnt from our mode of construction, and to still further reduce our estimate of the mean density of the sun.

Meanwhile we have to go into another long digression, with the view of trying to find out something about what the nature of the ether is or may be, which we think to be quite necessary before we go any farther.

CHAPTER XIII.

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THE ETHER A MATERIAL SUBSTANCE, PROVED BY ITS BEHAVIOUR.

WE have said in a former part of this work, pages 153 and following, that if the ether is capable of performing all the functions that are attributed to it, it must have some consistence or substance of some kind ; that it must be matter of some kind in some form, and consequently must have density in some degree however low ; and we might, for the same reasons, suppose that it must have some temperature ; but as long as we believe that without motion there can be no heat, we cannot conceive it to have any temperature. No doubt we might suppose it to be in a constant state of vibration, and to have the temperature corresponding to that state, whatever that may be ; but this, in addition to leaving us just where we were, would only entail upon us the task of supplying temperature as well as density to a body of whose existence no positive proof has hitherto been given, whatever we may believe about it. At the same time, the evident necessity of taking its temperature into consideration, seems to supply another reason for concluding that it is a material substance, over and above those we have cited now and before.

The general belief regarding the ether has been, ever since it was invented, that it is a substance of some kind (imponderable and impalpable?) which fills and pervades all space and matter; but a little consideration will show that this belief requires to be modified. The ether is supposed to be the connecting link of the universe, and the agent for carrying light, heat, electricity, and magnetism from the sun to the earth and planets, and all over space; but it has been found that electricity will not pass through a vacuum, such as has been produced by experimenters, unless it be with a very powerful current. This, of course, would seem to prove that there must be almost no ether in such a vacuum; because if there was ether in it, of the same density as there is in space, electricity would pass through it with the same ease as it does from one body to another on the earth or in space; it would seem, also, to justify us in inferring that electricity would not pass through an absolute vacuum at all, however powerful the current might be, because there would be absolutely no ether to carry it; and, likewise, that the quantity of ether remaining in the experimenter's receiver had as much to do with the passing of a very powerful current of electricity through it—perhaps a great deal more—as the small quantity of air, or gas, or dust not altogether exhausted from it, to which the experimenters attribute its passage. Moreover, it would appear that when air or any gas is pumped out of a receiver, the ether mixed with it is pumped out along with it; consequently it must be a material, tangible substance, possessing density in some degree, however low it may be. Here, then, we have, it would appear, proof positive that there is such a carrying substance as the ether has been supposed to be. It is a thing which we have not to conceive of, fabricate, or build up in our minds. It is a thing we can pump out of a tube, and is as much a material substance, in that respect, as air or any other gas that is as invisible as itself—yet nevertheless in the tube until it is pumped out.

Against this idea of the nature of the ether, and what may be done with it, it may be argued that light and heat pass freely through a tube or receiver *in vacuo*, when electricity refuses to pass; but are we sure that they do pass? It would be a much more difficult matter to prove that they do, than to prove that electricity does not, because our eyesight gives us evidence in the latter case. Besides, there are facts which, when thoroughly looked into, induce us to believe that light actually does disappear gradually from a vacuum as it is being formed.

In an article on "The Northern Lights," in "Science for All," Vol. II., reference is made to a well-known laboratory experiment in the following words: "We take a glass cylinder, covered at the ends with brass caps, one of which is fitted with a stop-cock, which we can screw to the plate of an air-pump. To the brass caps we now attach the terminals of a powerful induction coil, but as yet we perceive no result. We now begin to exhaust the air from the cylinder, and as the exhaustion goes on we soon see a soft tremulous light beginning to play about the ends of the cylinder; and this, when the air is sufficiently rarefied, gradually extends right through the cylinder. As we continue the exhaustion these phenomena will be reversed, the light gradually dying away as the exhaustion increases. We shall at once perceive how very much this resembles an aurora on a small scale, and so we have electricity suggested to us as the agent which produces the aurora." Farther on in the same article we find that: "Aurora displays usually take place at a great height—sometimes so high as 300 miles—while their average height is over 100 miles. At such heights the air must be extremely rarefied, and we should be disposed to expect that the electric discharge could not take place through it."

Now, at the beginning of this experiment, it must be granted that light was passing freely through the glass cylinder from side to side, and also that, when the electric current was turned on, the electricity was passing freely through the air in the cylinder though it was not visible. It could not pass through the glass on account of its being a non-conductor. Then, when the air had been partially exhausted from the cylinder, and the "soft tremulous light" began to appear about its ends, it is clear that some interference with, or change in, the free passage of light through it must have been produced, both transversely and longitudinally, which occasioned the difference in the appearance of the light and caused its tremulous motion. And as change in the appearance of the light extended through the length of the cylinder as the exhaustion increased, and finally died away—light, change and all—when it approached more nearly that of an absolute vacuum, we cannot help concluding that the light disappeared because there was no medium left in the cylinder, of sufficient density at least, through which it could pass; which, of course, means that light cannot pass through a vacuum any more than electricity can.

The experiment we have cited above may be considered antiquated, but similar results are presented to us in Professor Balfour Stewart's "Elementary Physics," where he says at page 399 of the Reprint of 1891: "Another peculiarity of the current is the stratification of the light which is given out when it traverses a gas or vapour of very small pressure. We have a series of zones alternately light and dark, which occasionally present a display of colours. These stratifications have been much studied by Gassiot and others, and are found to depend upon the nature of the substance in the tube." [The ether?] "If, however, the vacuum be a perfect one, Gassiot has found that the most powerful current is unable to pass through any considerable length of such a tube."

[In passing, we take the opportunity to assert, with confidence, that there can be no perfect vacuum on the earth.]

Here we see the gas or vapour in the tube divided into zones alternately light and dark, which occasionally present a display of colours, and are led to infer, from the colours depending upon the nature of the substance in the tube, that they disappear altogether when the exhaustion is sufficiently great ; and are finally told that the most powerful current is unable to pass through such a tube of any considerable length. In this case also, we can say with perfect confidence that there can be no ether left in the tube, in sufficient quantity, or else it would be able to carry the electricity through it much more easily than from the sun to the earth, or from one part of the earth to another. If we refuse to acknowledge that the ether has been removed from the tube or cylinder, we are forced to conclude that it is not the carrying agent, for which alone it has been called into existence by the imagination of scientists ; and we have to invent new theories, new methods for explaining what we have been accustomed to think we thoroughly understood. We have to look for a new dog to carry and fetch. Furthermore, all that has been said about electricity is equally applicable to light, whether we can prove it or not. If light could pass freely through the experimental cylinder from side to side, as it was certainly doing before the exhaustion was begun, we cannot understand why there should be, first tremulous light which finally disappeared, and why dark strata were displayed in it by the forced passage of electricity ; unless it was that the carrier of the light was removed, and then we naturally think of why there should be dark strata in the tube. We can understand electricity lighting up darkness, but not its darkening light—it lightens up midday—and we must conclude that both the one and the other were driven through the cylinder, or similarly conducted through it, by the same force, or were left behind.

Following up the quotations we have already made from "Science for All," Vol. II., we now add another for further illustration of what we have been saying, to wit : "Let us now return to the laboratory, and see whether we can make any experiment which will throw light upon this difficulty. If we send the electric discharge through one of the so-called vacuum tubes—choosing one which consists, through part of its length, of tube which is much narrower than the main portion—we find that when the discharge is passing the pressure is greater in the narrow part of the tube, showing that in some way gas is being carried along by means of the current, and Professor A. S. Herschel suggests that in some similar way air may be electrically carried up to these great heights." This quotation, of course, refers to the Northern Lights, but it serves to illustrate what we are seeking to show with respect to the ether.

In this experiment, the explanation of the pressure being greater in the narrow part of the tube, is exactly the same as that for water passing through a conduit which is narrower at one place than another. The same quantity of water has to pass through the narrow as through the wide part, consequently the velocity and pressure (head) have to be greater than in the wide part—the water arranges that for itself; and the seeming difficulty of explanation arose from the idea "that in some way gas is (was) being carried along by the current," when it was only the gas that was being lighted up more vividly by the electricity passing through it, because the same amount of electricity had to be carried through the narrow part as the wide one. No portion of the gas could be carried along with the electricity, else it would very soon have been all accumulated at one end of the tube, or a reverse current must have been set up to restore the balance, which would speedily have shown itself. Had the said tube been filled with copper instead of gas, the experimenter must have known that the electricity, in passing through it, would have spread itself all through the wide part, and contracted itself to pass through the narrow part, spreading itself out again through the other wide part, thus giving rise to differences of pressures and velocities at the different widths of the tube; but, of course, he would not have been able to see this, because the electricity could hardly be in sufficient quantity to light up the copper, or to impart to it sufficient heat to make it visible. Neither would the electricity carry with it part, or the whole, of the copper when passing through the narrow part. It would be the gas lighted up more vividly, not set in motion, by the electricity that the operator saw in the experiment under discussion, and, no doubt, if the tube had been sufficiently exhausted of gas, the light would have disappeared the same as in the first quoted experiment, and the electricity would have ceased to pass because there was nothing, in sufficient quantity at least, to carry it along, not even the universally commissioned monopolist the ether. Let us ask here: Does not all this seem to prove that electricity is a carried, not a carrying, agent?

In the quotation made, at page 229, from "Elementary Physics," we are told that when electricity passes through a gas or vapour of very small pressure, "We have a series of zones alternately light and dark." Now we ask, Why should part of these zones be dark? and the only answer to be given is—simply because there is no light in them, nothing in them to carry or hold light. Otherwise, we cannot understand why they should appear to be dark. We cannot imagine a glass tube with light and dark zones in it longitudinally—we have understood the zone to be longitudinal; transverse sections would not be zones—at the same time that light is passing freely through it transversely, i.e. from side to side, unless it is that in the dark zones there is nothing, not even the all-pervading ether, to carry or hold light in; therefore, we conclude again that there is no light where there is no ether.

For an explanation of the existence of light and dark zones in the almost exhausted cylinder or tube, we refer to Professor Tait's treatise on "Heat," where he says, in section 358, "What happens at exceedingly small pressures is not certainly known. In fact, if the kinetic gas theory be true, a gas whose volume is immensely increased, cannot in any strict sense be said to have one definite pressure throughout. At any instant there would be here and there isolated impacts on widely different portions of the walls of the containing vessel, instead of that close and continuous bombardment which (to our coarse senses) appears as uniform and constant pressure." Admitting the truth of the kinetic theory of gases, we can see that in a vacuum so rare that only electricity at a very high pressure could be forced (carried?) through it, we have the prescribed conditions in which there cannot be "one definite pressure throughout" the whole tube; in other words, we shall have some places in a vacuum tube where there is no gas at all, or perhaps even ether, and others where the gas is so rare that it takes a powerful stream of electricity to light it up in passing through, whether the lighted-up zones be composed of gas, or of ether, or part of both. If it did not pass, there would be no light-streak even. And further, we have to notice that the light and dark streaks would be changing places constantly, owing to the collisions of the small number of atoms or molecules of the gas, still not exhausted from the tube, driving each other from place to place.

All this makes us think of what is the real carrier of electricity through a partial vacuum, through a gas, or through a substance of any kind whatever, and we can only imagine it to be the ether. In that case the conductivity of any substance would depend upon the quantity of ether contained in it, and we can give no other reason for there being conductors and non-conductors of electricity. All matter has been thought to be pervaded by the ether, but we have said before that this must be the case in a limited sense only. It can be shown that glass is permeable to ether, and is therefore not an absolute non-conductor. Metals are supposed to consist of atoms bombarding and revolving around each other under the control of ether. Intermediate conductors may have the quantity in them of ether corresponding to their conductivity; and the compressibility of water, or any liquid, may depend upon the quantity of the ether mixed with its ultimate atoms.

Although we consider it to be going rather beyond the course we had laid out for ourselves, we cannot help returning to the article on the "Northern Lights" in "Science for All," quoted above in connection with electricity in the presence of a vacuum; because it helps to illustrate the subject we are dealing with.

In the regions where these Lights are seen, we know that there can be no want of ether, because it is supposed to pervade all space; but we know that there must be a very great want of air, or vapour of any kind, due to the height above the earth at which they are seen. Here, then, we have a great field for differences of pressures being caused all through it, by the collisions among themselves of the molecules or atoms of the extremely attenuated air; we have the higher or lower pressed zones of the laboratory experiment spread out before us, and if we suppose currents of electricity to be passed through them, we have an aurora in the high heavens, a counterpart of what was seen in the vacuum tube. The bombardment of the molecules continually shifting their positions and creating zones of different pressures, when lighted up by electricity, would easily account for the flashes, coruscations, and changes of the aurora; but, how does the air get up so high as is stated in the quotation at page 228?

We cannot accept the supposition of Professor A. S. Herschel that the air is carried up to the height of from 100 to 300 miles by electricity. We must believe, till evidence is given to the contrary, that electricity is a carried, not a carrying, power. Conductors of sound are all material substances; sound is not. It seems logical, therefore, to conclude that the ether is a material substance, because it conducts light, heat, etc. etc., which are not material substances. Proof is therefore required that electricity is a material substance, before it can be called a carrier. That air does somehow get up so high there can be little doubt, as is satisfactorily proved by the burning of meteorites when they come into our atmosphere at heights said to be more than 300 miles. How it does mount up so high is not so wonderful as it seems, when we take into consideration the causes of the trade winds, which are: The upward currents of the air created by the heat of the sun; the centrifugal force inherent in it at the time of leaving the earth; and its angular motion, which may be, at a guess, from 10 to 16 miles per minute, seeing that the equator has an angular velocity of over 1000 miles per hour. Then, from the time it leaves the earth, the air must begin to lose its angular velocity, the impelling power being cut off, and form a bank higher up, opposing the motion forward of all the air following it, so that immediately above the tropics there must be forward motion and obstruction, producing whirlwinds of which we can see or know really nothing, though they must exist, and which may carry air or vapours up to very great heights, carrying with them densities far beyond what would correspond to the simple attraction of the earth. At these heights this attraction would be very much diminished, and almost the only way in which the density of the whirlwinds could be diminished would be by expansion, which would not be very active in bodies already very considerably attenuated, as the whirlwinds would naturally be. Their movement towards the poles would be the same as that of the trade winds has always been supposed to be ; and we can now see how there can be air at great heights in the aurora regions, not carried up by electricity. In fact, the air may, or rather must, have carried the electricity up with it, as we shall, we believe, presently see.

We have not supposed that all the air, raised from the earth by the heat of the sun, is carried up to such altitudes and to its polar regions, but only a very small part of it ; and we have to add that there is perhaps not always electricity present in sufficient quantity to illuminate the air when it is carried up, which would, from the nature of its ascent, be undoubtedly divided into zones, streams, or belts at different degrees of tenuity. We do not doubt, or rather we believe, that electricity is always present in the atmosphere ; but we are not sure that it is always so in sufficient force to make itself manifest. A very homely example of this is : Stroke a cat's back in ordinary circumstances, and it will only arch it up in recognition of the caress ; but stroke it on a frosty night and it will emit sparks of electricity. The cat's hair does not shine—perhaps fortunately for the cat—because the electricity in it is not present in sufficient force, and only shows itself when the hand acting like a brush collects it into sparks. This shows not only that electricity is more abundant in the air at one time than at another, but that it is more so in cold and dry than in warm and moist air. It also shows one of the reasons why auroras of great brilliancy and extent are not continually in play in their own special regions, which is the want of a sufficient supply of electricity ; another reason being, the absence of the requisite zones, or masses of air in cyclonic motion at different pressure and in sufficient quantity. We understand from what we have read that the glow of the aurora is seldom awanting in clear weather in the far north, and can imagine that there is always a sufficient supply of electricity and attenuated air to maintain the glow constantly; and also that the brilliant displays are only made when there is a sufficient influx of whirlwinds of air at low and varying pressures, and of electricity in sufficient force to light them up. We should suppose that the bright flashes would take place where the pressure was greatest, and the illuminated darkness, so to speak, where it was least. Electricity does not carry up air to these heights, neither does magnetism bring it down from the sun; still a magnetic storm produces brilliant auroras.

Confronting these reflections with the laboratory experiment we have cited at page 228, we see that they are very fully confirmed by it; perhaps it would be more true to say that they were originated by it. When the current of electricity was first turned into the glass cylinder, no result was perceived. This must undoubtedly be construed into showing that the light in the cylinder, passing through it from side to side, was more powerful than the diffused light of the electricity passing through it from end to end; which was the reason why there was no result. By diffused, we mean that the electricity, turned into the cylinder through a thin wire, would immediately spread out over the whole of its width (or cross section) and thus very much weaken its light-giving power. When exhaustion had proceeded to a sufficient extent to produce the soft tremulous light, we can only conceive that the transverse light had decreased so far that the diffused light of the electricity, passing longitudinally, had begun to balance it, which caused the tremulous appearance on account of the one beginning to disappear and the other to take its place. And when the light extended through the whole length of the cylinder and the phenomena were reversed; and when the light died away altogether, when the vacuum became sufficiently pronounced; we can only believe that there was no light at all in it; neither natural light passing through it transversely, nor light of electricity passing longitudinally. Should any one object to this demonstration, as we may call it, we refer him to the quotation, made at page 229, from Professor Balfour Stewart's "Elementary Physics," and ask him, How could there be dark zones in a tube, through which light ought to pass freely from side to side? The thing appears to be tremendously absurd. There were dark streaks in the tube and other streaks of gas, or vapour of some kind at very low pressures (see also quotation from Professor Tait at page 232) that were lighted up to some extent by the current of electricity, but even these died away. We do not pretend to impugn the idea that the stratification of light and dark zones depended upon the nature of the substances in the tube, we only want to insist that the substances left in it were so extremely rare that electricity could not pass freely through it longitudinally, nor daylight transversely, else there could have been no dark zones in it; and that even the ether was in such small force that it could not perform the carrying duties assigned to it.

We have often wondered whether any experiments have ever been made to ascertain whether any changes, as far as the presence of light is concerned alone, have been brought about by producing a vacuum in a tube. The gradual dying away of light, and its final disappearance, are certainly suggestive of changes, and may have excited curiosity to know what actually happens. That there are changes cannot be denied, and it would be satisfactory to know what they are. It appears to us that one simple and easily made experiment would give a good deal of information on the subject. Let a glass tube of cylindrical form—one of those prepared for vacuum experiments—be placed in a slit in the window-shutter of a dark room, so that absolutely no light can pass into the room except through the hollow part of the tube; which might be effectually managed by burying two opposite sixth parts of its circumference in the wood of the shutter, and there would still be left one-third of its diameter for the free passage of light from side to side. When so arranged, and when still full of air, let a spectrum be taken of sunlight passing through it, to serve for comparison. Then let a high vacuum be produced in the tube, and another spectrum taken and compared with the first. This will at once show whether any change has been produced or not. Should the difference we expect be found, the experiment might be extended by spectra being taken at different degrees of exhaustion, from which some useful information might be derived.

We have said, at page 129, that the ether does not pervade all bodies of all classes, and such must be the case in some measure at least, otherwise there would be no non-conductors of electricity, no insulators for our electric telegraphs and deep sea cables. Were glass, for instance, pervaded freely by the ether, and the ether is in reality the carrier of electricity, then electricity could pass freely through glass, but it does not; therefore, there can be no, or at all events very little, ether in glass or any other insulator. We can see, then, the possibility of the ether being removed from a glass tube, provided it is a material substance, by shutting up one end of it with a stopper of glass and passing a perfectly-fitting glass piston through it to the other end. Suppose this done, it would be quite safe to say that electricity could not pass through the tube, because there would be nothing—absolutely nothing—to carry it, not even the piston-rod, for we could have that not only made of glass but on the outside of the piston. In this case the result would be exactly the same as when the contents of the tube were pumped out of it, and the residue left, if any, would be the same, that is, an immeasurably small quantity of the ether which had filtered through the glass. It may be argued that it would be impossible to make such an experiment as we have proposed, but that does not damage in the slightest degree the correctness of the consequences deduced from it; any more than the impossibility of constructing a perfect heat engine destroys the deductions drawn by Sadi Carnot, from the study of such an ideal machine. We can grant that glass being not an absolute non-conductor, the ether might, in course of time, ooze through it and fill the tube again, while gas, air, or dust could not so ooze through it, and thus re-establish the current of electricity that was stopped for want of it; but we cannot grant that there was any very perceptible quantity of ether in the tube, when the electric current could not pass through it without dismissing the ether altogether, and dropping back into the difficulties out of which it has in many cases lifted us.

The evident fact that the ether cannot pass through glass freely, and therefore cannot carry electricity with it, may be disputed by referring to the free passage of light, and also of heat, through glass and other substances, in virtue of transparency and diathermancy, two terms that have the same meaning, at least, as nearly as that light and heat mean the same thing; but we believe that this free passage, instead of invalidating our reasoning, only tends to prove that the ether is a material substance; because, if it is not, it might pass through transparent bodies just as easily as light and heat do. Of course, this belief obliges us to show how light and heat do pass through a transparent body such as glass, and the mode is exactly the same as of heat passing through any other body that is a conductor of heat. Glass is a substance that is known to be a bad conductor, but it is also known that it is not an absolute non-conductor of heat; therefore, there is no difficulty in supposing that it, and its companion light, can be conducted through glass with velocity proportioned to its thickness. We know that in the case of a pane of glass in a window it is practically instantaneous, but that does not mean that it is absolutely so. We know also, that in passing through, both are refracted, and that comparatively little heat is imparted to the glass, even under bright sunshine, which may be very well accounted for by the ether on the other side of the window pane carrying them (light and heat) off, in the same direction they were going, quite as fast as they could be conducted through the glass. But, supposing there was no ether in the room to which the window gave light, or gas, or elementary matter of any kind—a condition which could be obtained by making the room of glass and pumping out its contents as was done with the vacuum tube—What would be the result? There would be no wave motion to carry on light and heat into the room, and it would be in the same state as the exhausted tube, except that there would be no electricity in the room—no current being passed through it—nor anything in sufficient quantity to be lighted up if there was; the light would be stopped and reflected back from the glass, and nothing inside the room could be seen; not even that it was dark, because there would be no electricity to make dark zones visible. The window, or rather the whole room, would become a many-sided mirror, for reasons almost identical with those that account for a sheet of glass being made into a mirror.

We confess that all these deductions have startled us, but we can see no flaw in the reasonings which have led to them. If it is not for want of ether—in sufficient quantity at least—and the admission of variable quantity is to admit that it is a material substance, that electricity will not pass through a highly exhausted tube, we cannot imagine what can be the reason why it does not; simply accepting it as a fact is by no means satisfactory. In the dilemma between renouncing the ether altogether or acknowledging its disappearance—effective at least—it occurred to us that it might be for want of heat, and that in terms of the inter-dependency of temperature and pressure in a gas, heat disappeared in proportion to the decrease of pressure in the air or gas that was being exhausted from the tube, or from cold being applied to it from without; but that notion has already been disposed of by our own work, when we have seen that a gas in a close vessel can be heated or cooled to any degree, altogether independently of pressure.

When, acknowledging that the ether ought to have some temperature as well as density, we have said that it might have the temperature of vibration whatever that might be, thereby admitting that we could not pretend to determine what it is; nevertheless, we may take a look at it from a distance, and at least see what it cannot be, anywhere within the limits of our system. We have shown, at page 220, that when the original nebula was about 29,000,000 miles in diameter, its density must have been 0·179 that of air at atmospheric pressure, and its temperature $-225°$, and that these could be neither the density nor temperature of space. With this temperature, then, it is evident that there was still heat enough and to spare in the ether—considering it to be a material substance—to cause it to vibrate and perform its assigned offices; and, therefore, it could not be for want of heat that neither it, nor light, nor electricity could be carried through the vacuum tube, but for want of the ether in due quantity; consequently, the temperature of vibration cannot be so great as $-225°$. Turning back now to page 129, we find the density of the ether estimated at $\frac{1}{5{\cdot}264{,}800}$th of an atmosphere, which corresponds to an absolute temperature of $0{\cdot}000052°$ or $-273{\cdot}999948°$; but on the following page we expressed our opinion—well founded, we believe—that the estimate was too high, i.e. too dense, and that it might be 2, 3, or 4 times, or more, too great. Be this as it may, we can see that if the ether alone occupies space—beyond a comparatively very limited distance from any body belonging to the solar system—it must be almost absolutely free from temperature of any degree, for the difference between $-273{\cdot}999948°$ and $-274°$ is virtually nothing; or it must have a special temperature derived from the collisions of its own atoms, or from the sun. We have said more than once that the temperature of space cannot be so high as *minus* 225°, and now we cannot believe that it can be so low as absolute zero, because the ether in it is credited with the motion of vibration, which must be either the cause or effect of heat. What then shall we say? We can only speculate.

We can suppose that when the chemical elements were created, or evolved by some process, and began to attract each other, they had the ether to carry them into collision and produce heat; and that it, being also a material substance, became heated to the same degree as the other matter, always increasing in proportion to its state of condensation, the ether mixed with the other matter being also, of course, condensed. Then, following up this supposition, we can see that when the sun came to be condensed to its present state, the ether must have had the same degree of heat as itself at its surface, and be of the same density as it would in our air at the earth's surface condensed to the pressure of nearly 28 atmospheres; knowing as we do that the attraction of the sun at the surface of its photosphere is almost 28 times greater than that of the earth at its surface. Under this supposition, therefore, the ether might emit light just as surely as any other matter that may exist, or can be seen, in the corona or atmosphere of the sun, and might be the cause of the Zodiacal Light, probably more naturally than any other cause that has been imagined for it.

Mr. Proctor, in his "Sun," has given us a most elaborate description of how the Zodiacal Light could be produced by the swarms of meteorites and meteors, that are generally supposed to be floating around the sun and continually showering in upon it, and we confess that his reasoning is very plausible; but it, along with other similar hypotheses, has one very serious defect which it is hard to get over, under our existing ideas about matter and its origin. If there is a constant rain of meteorites and meteors falling into the sun now, and the same has been going on during the multitude of millions of years that it is supposed to have existed, we have to acknowledge that it must either come to an end some day, or that there is going on a constant creation or evolution of matter to keep up the supply. It will not suffice to accept the hypothesis that the supply comes from other suns, or any idea of that kind, because each one of them would finally find itself alone with its planets, etc., if it has any, in its domains the same as our sun. Neither would it suit the ideas of those who consider that matter has existed from all eternity and has *made itself* into all sorts of bodies or systems to suit them. Without continued creation, or evolution, matter must end in condensation into one mass. There can be no self-evolution to keep up the supply of matter. It would require another and exactly opposite power to unmake the final mass, and another change to original matter to start anew on the old course.

But we are speculating too soon. It may be said that if the Zodiacal Light is caused by the ether, and if the ether is a material substance, it must be exhausted sooner or later, just the same as all other matter and the whole universe to one mass the same as before; and also that we have no authority for supposing that the ether can be heated and cooled or condensed and expanded. But we think that with what we have done in this chapter, and what we will be able to show in the following one, we shall be able to get over all these difficulties, and also show how the universe might be dissolved and renewed by the ordained process of evolution.

CHAPTER XIV.

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THE idea that the ether can be pumped out of a tube of any kind, along with the air or gas that has been shut up with it therein, will very probably be declared to be absurd, by reference to Dr. Crookes's experiments with his Radiometer, and investigations into the nature of radiant matter ; but when duly considered his work seems to confirm it, and our reasonings in support of it, in a very convincing manner. Radiant heat, or light, is shown, no doubt, to penetrate into an exhausted bulb and to cause a radiometer to revolve, but we have to consider what is the state of exhaustion at which its force is shown to be greatest, and why that force decreases rapidly when the exhaustion is progressively increased beyond a certain point ; for a certain amount of exhaustion is required first of all to diminish the resistance of the air or gas to the vanes of the radiometer, before the radiant heat gathers force enough to make them revolve at all. Its greatest power to produce revolution is shown to be when the exhaustion is at from 30 to 60 millionths of an atmosphere, according to the gas or medium in the bulb—see "Engineering," Vol. XXV., page 155—and decreases from that point, often rapidly, as the exhaustion is increased, till at last it ceases altogether. Everybody who has taken any interest in the subject, knows that Dr. Crookes has exhausted radiometers to such a degree that they could not be influenced by the radiation of a candle placed a few inches from the bulb. We are not told at what degree of exhaustion this took place, nor at what degree repulsion, by radiation of heat, is supposed to have ceased altogether, but that does not matter, even though it should only cease when the vacuum comes to be absolute—most probably a stage to which it is impossible to attain. What concerns us is the fact that repulsion by radiation does reach a maximum at a certain degree of exhaustion, and then falls off as the exhaustion is increased; and what we have got to consider is what is the cause of the falling off. We are told it is caused by the attenuation of the matter, gaseous or material, contained in the bulb, and we are satisfied with the explanation. But in order to be thoroughly so, we must insist on believing that it is part of the whole of the matter that has been operated on; not only of the gas and other matter to the exclusion of the ether, but of the whole, ether and all. If the ether is left behind intact, it must perform the offices it was created for by the imagination of man, or man must discard it altogether. If it ceases to carry light and heat through a vacuum, it is of no more use than we found it to be in the case of electricity, and man is bound to dismiss it as a useless operative, who will strike work for no reason whatever. Some people have supposed the ether to be an absolute non-conductor of electricity, because it does not convey that agent through a vacuum. Will they also declare it to be a non-conductor of light and heat? If they will not, then they—and we presume everyone else—must admit that it can be pumped out of a bulb, in the same way as a gas or any other fluid matter.

Here we are led into another consideration, viz., whether the ether is exhausted from a receiver by pumping alone, or by the help of absorption. In his lecture, "On Radiant Matter," delivered at the British Association, at Sheffield, August 22, 1879, Dr. Crookes said: "By introducing into the tubes appropriate absorbents of residual gas, I can see that the chemical attraction goes on long after the attenuation has reached the best stage for showing the phenomena now under illustration, and I am able by this means to carry the exhaustion to much higher degrees than I get by mere pumping;" and that when working with absorbents: "The highest vacuum I have succeeded in obtaining has been $\frac{1}{20,000,000}$th of an atmosphere, a degree which may be better understood if I say that it corresponds to about the hundredth of an inch in a barometer column three miles high." (We quote from "Engineering," Vol. XXVIII., page 188.)

Now, what are we to think? Are we to suppose that the ether was in part removed by the absorbents? We think we are justified in saying that the absorbents had not anything to do with the exhaustion of the ether, because Dr. Crookes used different kinds of absorbents for the different kinds of gases he dealt with, and it is hard to believe that all the *media* he used were equally effective in absorbing the ether as they were with the gases. On the other hand, if we consider that the pumping was the only agent in removing the ether, we ought to acknowledge that it must have been more effective with regard to it than to the gases before absorption was resorted to with them; or that a stage had been reached at which the pump could not extract any more ether from the bulb. We shall have more to say of this presently. It is a difficult matter to determine, but there is one thing we can see clearly; when the exhaustion of the bulb was raised to $\frac{1}{20,000,000}$th of an atmosphere, the density of the ether—of itself—must have been at a lower degree than that. Consequently if we assume its normal density to be $\frac{1}{5,264,800}$th of an atmosphere, in terms of the estimate we quoted from "Engineering," it must have been diminished to less than one-fourth of that when the above high vacuum was obtained; because it must have been the density of the residual gas, or matter, and of the ether, added together which amounted to $\frac{1}{20,000,000}$th; the same as we have argued with regard to the solar nebula when at 6,600,000,000 and 29,000,000 miles in diameter.

One thing leads to another, and we have again to repeat our question—What is a gas? And all the answers we have been able to get to it hitherto have been far from satisfactory.

A little earlier in the same lecture, referred to a few pages back, Dr. Crookes, after telling us, very elaborately, what would have been the definition of a gas at the beginning of this century, goes on to say: "Modern research, however, has greatly enlarged and modified our views on the construction of these elastic fluids. Gases are now considered to be composed of an almost infinite number of small particles or molecules, which are constantly moving in every direction with velocities of all conceivable magnitudes. As these molecules are exceedingly numerous, it follows that no molecule can move far in any direction without coming in contact with some other molecule. But if we exhaust the air or gas contained in a close vessel, the number of molecules becomes diminished, and the distance through which any one of them can move without coming in contact with another is increased, the length of the mean free path being inversely proportional to the number of molecules present. The farther this process is carried, the longer becomes the average distance a molecule can travel before entering into collision; or, in other words, the longer its mean free path, the more the physical properties of the gas or air are modified."

Of course, what we have looked upon as Dr. Crookes's definition of a gas, ends with the second sentence of the above quotation, and is far from being sufficiently complete to be satisfactory; but we have continued to quote from the lecture, because it contains matter which demands consideration, and helps very powerfully to support the conclusions we have been arriving at.

Why the definition is not satisfactory, is that it does not tell us what there is in the spaces between the molecules of what is called the gas. If there is room for them to move in every direction there must be spaces between them, and these spaces must either be absolutely empty, or filled with something. If they are supposed to be empty, then the molecules being actually small pellets, like diminutive marbles, or snipe-shot, we immediately begin to think why gravitation does not make them, being ponderable bodies, fall down to the bottom of the bulb; and seeing that, by the definition, they are evidently considered not to do so, we think of what can keep them from falling, and of how they can be pumped out of a bulb or any sort of vessel. If we fill a vessel with marbles, snipe-shot, wheat flour, or dust, and set a pump to work on it, we shall find that we make very little progress in pumping them out of it. At first we might extract a puff or two of flour or dust—marbles or snipe-shot by no means—carried into the pump by any air there might be mixed with them, but that would very soon come to an end; besides, there would be air, gas, something, in the interstices—if any—of the flour or dust to drive them into the pump when a vacuum was formed in it, and the puffs would cease when the air, which would be in exceedingly small quantity, was all extracted. But independently of all this, we have supposed the spaces between the gas pellets or molecules to be absolutely empty, and there would be nothing to push them into the vacuum created in the pump. There is no possibility of pumping marbles, sand, flour, or dust out of a vessel without the assistance of a fluid agent of some kind, water, gas, or air; and even then it would be done with much difficulty.

Let us, then, suppose there is some such agent filling the spaces between the atoms of the gas and think of what it must be. Were we to ask the question we have a strong suspicion the first impulse of many people would be to reply—With gas of course. But this reply could not satisfy us. We should immediately be led to think of that gas also consisting of atoms with vacant spaces between them filled with something—some more gas; and were we to follow up that thought through a sufficient number of stages, it is easy to see that in the end the whole space occupied by any gas would come to be filled up with its own solid atoms, without any empty spaces between them through which they could move; and so rendered quite incapable of pushing each other into a vacuum formed by any pump that might be applied to extract them from any vessel of any kind; or we must suppose that each particle would fly of its own good will into the vacuum made by the pump—as it were on the wings of the morning. But we recall to memory that the wings of the morning do not always carry us to rest, and we see that filling the spaces with gas would only end in choking up the vessel altogether. It might be said: Nobody imagines that the molecules of the gas in the spaces would be sufficient to fill them up altogether; and then we have only to ask, What then would there be in the spaces between the molecules of these successive gases to prevent the whole of them from gravitating to the bottom of the vessel? And to add that there would still be empty spaces left, absolutely empty, that would have nothing in them to help in any way to force the molecules or atoms of any gas or vapour into a vacuum anywhere. It is clear then that a gas, such as Dr. Crookes has described a gas to be, could only end in filling the spaces left between its molecules or atoms. It would be an obstruction to their collisions and bombardments which form an essential part of the description or definition.

We must, therefore, have recourse to something else for filling up the spaces between the molecules of a gas, and the only thing we can lay hold of is our *limited liability* agent the ether, which we allow to do all we want it to do and nothing more. Vapours of solid or liquid matter would be of no use, for they would only condense into solid or liquid matter; unless always maintained at their temperatures of evaporation or ebullition, and that would at the best be only another form of a gas—nobody would use a liquid to assist in pumping air out of a vessel—and, besides, we should still have to show what keeps their particles apart, what fills the spaces between them, which would force us to appeal to the ether as the only source, just as before. If there are no spaces between the particles there can be no vapours.

If by pumping air out of a close vessel the number of its particles is diminished, and we acknowledge that the ether pervades all space and matter, in a greater or smaller degree, then we must either recognise that a pump is able to separate the particles of the air from the ether which pervades it in the vessel, and extract them alone ; or we must acknowledge that along with the particles of the air, the pump extracts a corresponding portion of the ether. Which of these two consequences of the pumping we have to choose cannot for a moment be doubtful. It would be as reasonable to suppose that we could pump the colouring matter out of a pond of muddy water, or the mud itself, and leave the clear water behind, as to suppose that the molecules of air, or of a gas, could be extracted from a close vessel, by a pump, and the ether left behind in it.

We have called attention two or three pages back to the fact that a fluid agent of some kind is required, in order to be able to pump matter of any description out of any kind of vessel. For solid matter a non-elastic fluid will suit, but for gaseous or vaporous bodies an elastic fluid is required ; but we have just seen that what have hitherto been considered to be elastic fluids, that is, gases and vapours, have no elasticity whatever of their own, but are undoubtedly and in reality solid matter ; and that in order to become elastic fluids they have to be mixed with the ether, or something that has yet to be discovered, invented, or imagined. If, then, until such a body is found we take the ether as a substitute, we have to acknowledge that it must be not only an elastic fluid but a material substance, capable of being compressed and expanded, and heated and cooled ; for nobody could conceive clearly the existence of an elastic fluid that is not subject to these conditions. He could not understand how the molecules of a gas could be contracted, expanded, heated and cooled in a vessel, while the elastic fluid which gave them liberty to move or to be moved, remained constantly at one density and temperature. Furthermore, until such a substitute is found, we have to acknowledge that it is the only thing we have any idea of corresponding to a gas as described by Dr. Crookes ; that is, a multitude of molecules colliding with and bombarding each other or their prison walls. But even beyond this we can uphold it to be the only real, independent gas there is ; because, being an elastic fluid, there is no necessity for there being empty spaces between its molecules, or even having molecules in the common acceptation of the term. We have no reason to think that there are empty spaces between the molecules or particles of india-rubber ; and if there are, the ether is the only substance we can properly conceive them to be filled with.

The law of Avogadro is, that "Equal volumes of gases and vapours contain the same numbers of molecules, and consequently that the relative weights of these molecules are proportioned to the densities." Therefore we must always bear in mind that it is the *weights*, not the *volumes*, which are equal, and that the volumes may be very different. On this earth of ours, then, we may say with certainty that an atmosphere of gas is composed of a definite number of its special kind of molecules, mixed with a definite quantity of the ether, in such proportion that the sum of their densities shall be equal to the density of the air, at atmospheric pressure at sea level, and at 0° of temperature. Holding this belief, we can see that each molecule, or rather atom, of each gas must have its own amount of displacement to enable it to float in the ether with which it is mixed. This would account in the most satisfactory manner for the diffusion of gases, whereby any molecule, or atom, may float wherever it is driven by collisions with its neighbours, be it above, or below, or on a level with, a molecule of a lighter or heavier gas. Therefore, were it possible to determine with sufficient accuracy the dimensions of the atoms of all gases, perhaps even of a limited number of them, it would be possible to calculate the real density, or specific gravity, of the ether.

We have not forgotten that when, by pumping, the ether was reduced to at least one-fourth of its normal density, its buoyant power would be reduced in the same proportion, nor that, when in a state of rest, the displacement of a molecule, which enabled it to float in the ether, would not be sufficient to make it float at one-fourth of that density ; but it might be supposed that when so far relieved from pressure, the molecule could expand in proportion to the relief, especially if its form were that of a vortex ring, or of a hollow sphere. However, should this supposition not be admissible, we shall see presently that it is not necessary. We know that as long as any degree of heat remains in a gas collisions of its molecules will continue, dependent on their attraction for each other, which may drive them to any part of the containing vessel; and that it can only be when they are cooled down to the absolute zero of temperature that they can come to be at rest. But as we believe that the ether can never be reduced to this absolute absence of temperature, nor completely extracted from any vessel, we cannot acknowledge that the molecules of any gas, left along with it in the vessel, could ever come to be absolutely at rest, even although the molecules did not increase in volume with the diminution of pressure. And we think this conclusion will agree with the opinion of Professor Tait, expressed in the quotation, made at page 232, from his work on "Heat," where he says: "In fact, if the kinetic gas theory be true, a gas whose volume is immensely increased, cannot in any strict sense be said to have one definite pressure throughout." This, of course, is tantamount to saying that the diffusion of gases cannot continue to be always exactly regular at extremely low pressures, and must vary as the vacuum is increased; so that the volumes of the atoms and consequent displacements may continue always the same under all pressures. We see, then, from this quotation, that in all probability the molecules of a gas are not always equally buoyed up by the ether in a high vacuum; which very likely is the reason why there are dark streaks in it; streaks without any visible molecules of gas in them, because the ether was not dense enough to keep them afloat.

We have still something to add in support of what we said, at page 238, of glass not being pervaded by the ether, in the common acceptation of the word, and of our acknowledging that the ether might, in the course of time, ooze through it and fill up the bulb again, while air, gas and dust could not so ooze through it—nor even the larger particles of the ether; should we be forced to acknowledge that it consists of particles.

In one of a series of articles in "Engineering," Vol. XXV., on Repulsion from Radiation, we find, at page 155, what follows: "With the same apparatus, Mr. Crookes conducted a long series of experiments for determining the conductivity of the residual gas to a spark from the induction coil. In air he found, at a pressure of 40 millionths ($\frac{1}{25{,}000}$th) of an atmosphere, which will be seen from the diagram, is the pressure at which the force of repulsion is at a maximum, that a spark whose striking distance at the normal pressure of the atmosphere is half an inch will illuminate a tube whose terminals are 3 millimetres apart. By pushing the exhaustion farther, the half-inch spark ceases to pass, but a one-inch spark will illuminate the tube, and as a vacuum is approached more electromotive-force is required to force the spark to cross the space separating the terminals within the tube, until at still higher exhaustion a coil capable of giving a 6-inch spark in air at the pressure of the atmosphere is required to show any indication of conductivity in the residual air. It was found, however, in experimenting with so powerful a spark that occasionally the glass was perforated by the discharge taking place through the bulb; but it is a remarkable fact that the perforation in such cases was so excessively small that several days were occupied before equilibrium of pressure was established between the inside and outside of the bulb."

Here we notice first—and it was the reason why we have made the first and longest part of the quotation—that the spark whose striking distance was half an inch at the normal pressure of the atmosphere, fell to under one-fourth of its power in a vacuum of only $\frac{1}{25{,}000}$th of that pressure; that when a one-inch spark was required to illuminate the tube, it must have decreased to one-eighth in a vacuum of $\frac{1}{50{,}000}$th; and, if it be admissible to follow the same proportion, the 6-inch spark must have been exhibited in a vacuum of $\frac{1}{1{,}250{,}000}$th of an atmosphere at least. Perhaps all this experiment was carried on in *vacua* produced by pumping alone, and the final vacuum may have reached a greater height than that which we have just mentioned; but the most interesting part of it is the perforation of the bulb by the 6-inch spark.

In it we have to consider what was the conveyer which carried the electric spark through the glass of the bulb, instead of to the other terminal of the coil so close at hand, and it is a very difficult problem to solve. We naturally recur for some solution to the stratification of light given out when an electric current traverses a gas at very low pressure and gives rise to zones alternately light and dark as noted in the reference we made, at page 229, to Professor Balfour Stewart's experiments. We cannot think it unreasonable to suppose that the dark zones contained no matter at all that could be lighted up, and that it was the lighted zones alone which contained carrying matter for the electricity. If so, we can easily imagine one of these zones or strata carrying the perforating spark from the induction terminal to the nearest part of the glass of the bulb, for it was as possible for it to lie in that direction as in the direction of the other terminal, and the difference of distance between the first terminal and the glass, and between the two terminals, would not be so great as it appears to be on simply reading the accounts of the experiments; but we have still to think of how it managed to force itself through the glass of the bulb.

To get over this difficulty, we can refer to what we have said, that is, that glass may be thoroughly pervaded by the ether in an almost infinitesimal degree, and suppose that the electricity may have discovered, or rather been led to, the ether contained in the glass tube or bulb, and so found its way to one of the oozing holes we have said might exist in the glass; even the oozing hole may not have passed quite through the glass, and there might remain a very thin film to be burst open before perforation was complete. Also we may note that the zone which performed the office of carrier to the side of the bulb was much more probably composed of residual ether than residual air or gas, or at the least formed a preponderating part of the carrying element. The fact of the hole being so minute "that several days were occupied before equilibrium of pressure was established between the inside and outside of the bulb" on such occasions, goes far to prove that the carrying agent through the glass must have been the natural carrier of electricity, light, and heat. We cannot conceive that an eruptive force could open such a small passage through the glass of the bulb, but we can conceive that it should be able to force itself through a very minute passage already open, and even join two or more such passages into one. This conception makes us think of the many oozing passages there may be through a glass bulb; passages so minute that the ether might pass through them, but nothing so gross as any of our known gases; in fine, so minute that glass, for all the compact look it presents to us, may be only as a very fine sponge in respect to the ether. However, that the perforations related in the above quotation were large enough for air to pass through them there can be no doubt, otherwise the equilibrium between the pressures on the inside and outside of the bulb could not have been re-established even after many days; for there still remains the idea that the oozing holes might be so small that nothing but the ether could pass through them.

Should the glass of a vacuum tube or bulb be pervaded by the ether in the manner we have supposed it to be, and we believe there can be no doubt that it is so, it is obvious that its glowing when a current of electricity is passed through it must be caused by the electricity and consequently of its light, being carried into the body of the glass by means of the ether imbedded in, and forming a constituent part of, it. In connection with this we have to remember that the air in the tube does not glow when it is at full atmospheric pressure, but only when a certain degree of vacuum has been produced in it; and therefore it is equally obvious that it is only when the ether enclosed in the tube is reduced to the same degree of tenuity as that imbedded in the glass forming the tube, that the light of the electricity can be carried by it into the glass and make it glow. But to show this more clearly, it is necessary to refer to the steps by which we believe we have made very plain what must undoubtedly be the nature of the ether.

(1) First of all we have shown that, if there be such a thing as the ether, it can be pumped out of a close vessel of

any kind ; which proves that it must be a material substance, and in consequence can be expanded, or rarefied, and compressed the same as any other material substance ; and that if there is no such thing, something else, having these qualities, has to be invented to take its place. (2) In showing this it has been made abundantly clear by the example of the hair of a cat in variable weather, to which we may add the exhibition of lightning in daylight, that it cannot make electricity visible, or illuminate any matter, unless the quantity of electricity it has to carry bears some certain proportion to the density of the ether in the matter that is illuminated. (3) In proof of this we have shown how, through its carrying power it can convey electricity of adequate force up to very great heights, so as to illuminate very rarefied air and cause auroras ; the conveying being done either directly from the earth or by means of the ether mixed with the air carried up by whirlwinds to those great heights ; and (4) how electricity is carried into the body of a tube of glass and makes it glow.

With these examples we can extend our ideas to other exhibitions of light, which, otherwise, we could hardly avoid looking upon as mysterious. We can see how marsh gas, rising up from boggy ground, becomes mixed with common air till it reaches a certain density, and forms the Will-o'-the-wisp when there is sufficient electricity in the air to make the diffused marsh gas visible, through the medium of the ether always mixed with it ; or, perhaps, rather when the density of the diffused gas corresponds to the density of the ether. Then we have the phenomena of films of matter on the surfaces of certain liquids glowing with appropriate colours ; which films must be pervaded by the ether in proportion to their conducting powers, the same as we have seen must be the case with all kinds of matter, the light given off corresponding as is natural to the composition of the films ; and of course this same reasoning, or exposition, applies to the films formed on, or near, the surface of the sea which produce what sailors call "fire-on-the-wave." Lastly, and akin to the glowing caused in a vacuum tube, we cite the case of the glow-worm, the radiation from which must of necessity contain a certain amount of the ether in it, and may either glow constantly or intermittently according to its capacity for carrying electricity or light of any kind, constant or inconstant. Or if there is no radiation from it, its skin may possess the properties of a film on the surface of a liquid. We have seen in the "Times" of September 21, 1896, in its report for that day of the Meeting of the British Association at Liverpool, that in experimenting with glow-worms Dr. Dawson Turner had found some difficulty in getting them to glow when he wanted, but found they gave off the radiation whether glowing or not. Perhaps his interference with them destroyed the balance of force between the electricity present and the density of the ether in it without stopping the radiation.

Hitherto the light given out by a nebula, and any light of the kind not easily accounted for, has been attributed to incandescent gas not burning or being consumed, but only glowing. Now it is time to look upon it as belonging, at least in part, to the ether, and to look upon the bright line in the spectrum of a nebula as the *Ether Line.* We shall have to return to this later on.

We said, at page 248, that a fluid of some kind, elastic or not elastic, is necessary to enable us to pump solid matter out of a vessel of any kind, and went on to show that a gas as described by Dr. Crookes, or that can be described, in its own independent state of existence, by anybody, could not supply the want ; because it consists of particles, molecules, atoms—any name that can be given to them—which have no power in themselves to move or to give motion to anything ; they can be moved but cannot impart motion to anything, even to one another, until they are first set in motion by attraction. This in its turn led us to see that the only elastic fluid we have is the ether, and our work since then has taught us that we were wrong in saying at page 250 that a non-elastic fluid would suit for pumping solid matter out of a vessel ; for we now see that what we have been in the habit of looking upon as non-elastic fluids, must owe their fluidity, such as it is, to the ether, which, in proportionate degree, pervades them the same as it does all other matter. In this way we are run down to the only conclusion we can come to, namely, that the ether is the only connecting medium and carrying agent of matter that we have, or even initiator of motion, except attraction ; and being matter of the nature of an elastic fluid, there is no reason why we should not at once consider it to be attraction itself. It has been looked upon, for no one can exactly tell how long, as the connecting mechanism of the universe, thus having, in reality, assigned to it the attributes of the law of attraction, and all that we have to do is to put it in its right place. We are, in a manner, taught to look with suspicion on two agents being required to do one kind of work, or even two kinds of work that are so closely allied that we cannot separate them in a way that satisfies us ; and this is precisely a case in which we can have one agent that can connect matter, and at the same time carry immaterial elements from one place to another.

Having got this length we have still to go one step farther. We cannot now doubt that the ether is a material substance, and if it is, there is nothing to prevent us from considering it to be the primitive matter ; in fact it would be absurd to look upon it in any other light. We cannot conceive of anything having been created before the ether, or ordained before the law of attraction, and thus we have the two coeval and one. It is long years since physicists, chemists especially perhaps, began to think that the great number of chemical elements cannot all have existed from the beginning of things, and that it is far more probable that they have all been evolved from one primitive substance, and this idea must now be gathering more strength from day to day in view of the new elements that are being constantly discovered ; the unknown is being made known, and the air we breathe instead of being one in four elements, as in former times it was considered to be, is now not far from double that number in one. Adopting this notion, then, the ether is much more likely to have been the primitive element than any other material substance that can be thought of. If it has never been thought of in this light, it has come to be very remarkably near it, as may be seen by referring to the long quotation we made in Chapter

VII., beginning at page 129, where the idea of the ether being the connecting *medium* of matter is made use of to compute its density. Little thought we of this when we made the quotation, but there was the idea whether the author saw or not all that was implied in it.

Having broached the notion of the ether being the primitive element of the universe, or at all events, of the solar system, we might be expected to show how all the other elements were formed from it; but that has been done for us in a very much more able manner than we could have done it. Anyone who chooses to refer to "Nature" of September 2, 1886, will find—in Dr. Crookes's opening address, on Chemical Science in Section B, at the meeting of the British Association for that year—a very detailed explanation of how all the chemical elements might have been elaborated from one that he called Protyle ; in which explanation he will only have to change this word into Ether to comprehend the process much more easily than by any exposition we could pretend to draw up. To quote the whole address would be altogether out of place, and besides, our notes of it are only fragmentary. But for present satisfaction of those who cannot immediately refer to "Nature," we may say that in the same report it is clearly stated that Sir George B. Airy was of opinion that all bodies may not be subject to the law of gravitation ; and have no cause to think it strange we do not see that, were the ether and attraction one and the same, the whole universe would be finally collected into one mass, itself included. They will have better authority than ours for believing that the ether may connect matter evolved from itself, without being materially confounded with it. At the same time we acknowledge the necessity for expressing our idea of what we consider to be its nature, and in compliance with this obligation we say we have conceived it to be of the nature of indiarubber, not an elastic fluid as we have called it before, but rather an elastic substance like a jelly, as some people have conceived it to be; not a gas, because it does not require any medium to connect its particles.

Looking upon it in this light, action at a distance can be accounted for in a very natural manner. When a stretched indiarubber band is relieved from strain, the relief must be felt instantly throughout every part of its length; for, although the band may take time to contract, no time is required for the relief from strain being felt. In like manner an alteration in strain between the sun and the earth—and these alterations of strain are taking place every instant—connected by an indiarubber ether will be felt instantly in both bodies; and should anyone stand out for time being required to convey the attraction, let him remember that the difference of its power would be felt first at the two ends of the connecting medium, for the very good reason that even attraction itself could not prefer one extreme to the other. And that is all that is meant by action at a distance.

Here are some other things that could be explained more easily than they can be at present, through the ether and attraction being considered to be one and the same, than under any other conception we can form; but although we have a dim vision of such explanations in some cases, our knowledge of the sciences involved in them is not sufficient to warrant us in letting our dim conceptions see the light. Therefore all that remains for us to add is, that some things we have said of the ether may have to be so far modified now, but as they have had their part in leading us to the conclusions we have arrived at, they cannot be altogether suppressed.

CHAPTER XV.

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In this chapter we proceed to consider how the original nebula was formed, and whether the solar system could be evolved therefrom in the manner shown in the analysis of Chapter V.

The usual way of treating the solar system has been to suppose it to have been formed out of a nebula extending far beyond the planet Neptune, generally in a vague way; although some writers have specified a limit to the distance, in order to give some definite idea of what must have been the density of the nebula at some particular period of its existence. In the first part of our work we have adopted the same plan and we mean to follow it out, because it gives us a greater degree of facility for expressing our ideas, and making them more intelligible, than by adopting a new method. But we shall previously endeavour to show where the nebula itself came from and how it was formed, which seems to us to be as necessary as to show how it was transformed into the solar system.

We understand Laplace to have supposed the nebula to have been formed out of cosmic matter in its simplest condition, and in its most primitive atomic state, collected from enormously distant regions of space by the power or law of attraction. In this we shall follow him, because we do not see the necessity for matter having to be created in the form of meteorites or meteors, or any other form, to be afterwards dissociated and reduced to the atomic state, by heat produced by collisions amongst the dissociated atoms. Surely it would show more prescience, more simplicity of work, and economy of labour, to create matter in this primitive state, than in one which required it to be passed through a mill of some kind, as it were, before it was manufactured into nebulous matter; in fact, to make brickbats in order that they should be afterwards ground down—dissociated—into impalpable powder, to render them fit to be worked up into bricks. But our first effort will be to attempt to define the collecting grounds of this cosmic matter, somewhat more particularly than has been done hitherto, as we believe that even a superficial study of them will assist us greatly in forming a more comprehensive idea of the whole solar system than anything we have met with in any of the books which we have had the opportunity of applying to for information.

The collecting grounds, then, are clearly the whole region of space to which the attractive power of the sun extends, or what astronomers would call within the sphere of his attraction. These domains, like those of any other proprietor, are limited by the domains of his neighbours. At first sight, it would seem that his neighbours are infinite in number, but a little thought will show that the number may be very limited indeed. On this small earth of ours, it is a very common thing for a landed proprietor to be able to look over the domains of his neighbours, and see those of proprietors more remote; even to look over the domains of his neighbours' neighbours, and see properties so remote that he does not even know to whom they belong nor how they are named. With much more reason, the same must be the case with the sun, more especially as he, from his own mansion-house, sees nothing of the domains, but only the mansion-houses of others, there being no landmarks, hills, fences or woods to cut off his view, as there are upon the earth; the only interruption possible to his view being that another mansion-house should come to be exactly between his and that of a farther-off neighbour. For our purposes, we will assume that his nearest neighbours are those the distances of whose mansion-houses have been measured, and will adopt the following list of them, taken from Mr. George Chambers's "Hand-Book of Astronomy," part 3, page 10, 5th edition, 1889, and forming Table VII. All that we can learn from this table is that the boundary between the sun and any one of the stars mentioned in it must be somewhere on a straight line connecting the two, but that does not furnish us with any information as to the extent of the sun's domains, although it does help to give us some idea of their form. For some knowledge of their extent, we require to know how far the lordship of each one of the proprietors extends from his mansion-house; which, very much the same as it does upon the earth, depends upon the power he has to take and keep it; it depends on the mass of each neighbour who actually marches with the sun when compared with his own mass. The list referred to does not help us in any way to determine this, as we have just said, but we have found in Professor Charles A. Young's "Lessons in Astronomy," of 1891, page 270, the masses of six binary stars whose distances, calculated from the parallaxes given in it, furnish us with data from which we can calculate the distance from the sun of the boundary between him and any one of them. The number is very small, but still from them we can gain some notion of what was the form of the domains from which the original nebula was collected; that is, always under the supposition that the sun and his system were evolved from

TABLE VII.—LIST OF STARS WHOSE DISTANCES FROM THE SUN HAVE BEEN MEASURED, AND WHICH ARE ASSUMED TO BE HIS NEAREST NEIGHBOURS.

Star.		Magnitude.	Proper Motion.	Parallax.	Distance.		Observers.
					Sun's Distance = 1.	Time of its Light reaching the Earth.	
			″	″		years.	
α	Centauri	1	3·67	0·75	275,000	4·34	Gill.
61	Cygni	6	5·14	0·50	412,500	6·51	O. Struve.
21185	Lalande	7¼	4·75	0·50	412,500	6·51	Winnecke.
	Sirius	1	1·24	0·38	543,000	8·57	Gill.
μ	Cassiopeiæ	..	..	0·34	606,000	9·57	O. Struve.
34	Groombridge	8	2·81	0·29	711,000	11·23	Auwers.
9352	Lacaille	7½	6·95	0·28	737,000	11·62	Gill.
21258	Lalande	8½	4·40	0·26	793,000	12·52	Krüger.
Ö	Arg. 17415	9	1·27	0·25	825,000	13·02	Krüger.
σ	Draconis	5	1·87	0·25	825,000	13·02	Brunnow.
ε	Indi	5¼	4·68	0·22	938,000	14·80	Gill.
α	Lyræ	1	0·31	0·20	1,031,000	16·27	——
o²	Eridani	4½	4·10	0·17	1,213,000	19·15	Gill.
ρ	Ophiuchi	4½	1·00	0·17	1,213,000	19·15	Krüger.
ε	Eridani	4½	3·03	0·14	1,473,000	23·24	Elkin.
ι	Ursæ Majoris	3	0·52	0·13	1,586,000	25·04	C. A. F. Peters.
α	Boötis	1	2·43	0·13	1,586,000	25·04	C. A. F. Peters.
γ	Draconis	2	0·06	0·09	2,292,000	36·17	——
1830	Groombridge	7	7·705	0·09	2,292,000	36·17	Brunnow.
	Polaris	2	..	0·07	2,947,000	46·50	C. A. F. Peters.
3077	Bradley	6	2·09	0·07	2,947,000	46·00	Brunnow.
ς	Foucani	6	2·05	0·06	3,438,000	54·25	Elkin.
85	Pegasi	6	1·38	0·05	4,125,000	65·10	Brunnow.
α	Aurigæ	1	0·43	0·04	5,157,000	81·37	C. A. F. Peters.
	Canopus	1	..	0·03	6,875,000	108·50	Elkin.

a nebula. From these data, Table VIII. has been drawn up, which shows the distances of the six stars from the sun, and the limits of his sphere of attraction in relation to them expressed in terms of radii of the earth's orbit, and also in radii of Neptune's orbit, which gives numbers more easily comprehended by us.

TABLE VIII.—MASSES OF A FEW BINARY STARS SHOWING THE LIMIT OF THE SUN'S SPHERE OF ATTRACTION WITH RESPECT TO THEM, IN RADII OF THE EARTH'S ORBIT, AND DISTANCES OF THEIR BOUNDARIES WITH THE SUN IN THE SAME MEASURE, AND ALSO IN NEPTUNE DISTANCES.

Name of Star.	Parallax in Seconds	Mass. Sun's Mass = 1.	Distance of Star from Sun in radii of the Earth's Orbit = 93,000,000 Miles.	Distance of Limit of Sun's Sphere of Attraction in radii of Earth's Orbit = 1.	Distance of Limit in radii of Neptune's Orbit = 2,794,000,000 Miles.
	"				
α Centauri .	0·75	2·14	275,000	128,505	4,277
61 Cygni . .	0·43	0·23	479,686	369,358	12,294
Sirius . .	0·38	4·26	543,000	127,465	4,243
α Geminorum	0·20	0·30	1,031,325	721,927	24,030
70 Ophiuchi .	0·16	5·00	1,289,150	257,830	8,582
η Cassiopeiæ .	0·15	3·00	1,375,100	458,366	15,257

But there is still something to be said with respect to the Binary Stars of Table VIII., and any others whose masses may be met with later on. If those forming a pair revolve around each other, or a common centre, in orbits, it must happen that they will be sometimes more or less in conjunction, opposition, and quadrature with regard to the sun; also the angles of the planes of their orbits to direct lines between them and the sun, whatever these angles may be, will cause variations in the separate and combined forces of attraction they exercise in the domains of the sun, at different periods of their revolutions; so that these powers of attraction will be constantly increasing and diminishing, and causing the boundaries of their domains to approach and recede from the sun; thus introducing between their domains and those of the sun a debatable land, which will reduce celestial to be very much like terrestrial affairs, where each proprietor, or power, takes the pull when an opportunity presents itself. No doubt all such invasions, or claims, between proprietors will be settled by the law of attraction, without lawsuit, arbitration or conflict ; but as law gives right, and might is right —most emphatically in this case—we come back to the old seesaw of earthly matters. Well, therefore, may astronomers teach that the whole universe is formed out of the same kind of materials, and governed by the same laws that we are having good reason to know something about on this earth of ours.

Accustomed to look upon *α* Centauri as the star nearest to us, on account of its light-distance being so much smaller than any other noted in our text-books, we were not prepared to find that, when measured by his sphere-of-attraction distance, Sirius is actually a rather nearer neighbour to the sun than it ; nor that his, apparently, next nearest neighbour, when measured in the same way, is twice as far away as either of them ; and thus we have the conviction thrust upon us that they must have made deep hollows in the solar nebula when it was being formed. On the other hand, when we think of three of the other stars mentioned in the list of six, being practically from three to six times farther off than either of them, we come to the conclusion that the form of the nebula, when in its most primitive state, must have been of a very jagged character ; a conclusion which is very considerably strengthened when we look at Table VII., and see that the stars noted in it run up to from twice to not far from thirty times more distant from the sun than *α* Centauri. And now, having got a somewhat definite idea of the form of the sun's domains, we may attempt the construction in them, first of a nebula and afterwards of a solar system, such as our text-books describe to us ; introducing into the construction, as a matter of course, the variations from existing theories which, we believe, we have demonstrated to be necessary.

Perhaps we ought to confine our operations to these domains, and so we will almost exclusively ; but the sun has been so long considered as one of many millions of stars, and as part of what is now looked upon as our universe, that we cannot help looking upon the whole as having been the result of one act of creation ; more especially as we have no reason whatever for supposing it to have been built up piece by piece ; and whatever ideas we may form of our own little part of it, we are bound to apply them to the whole. We may, therefore, lay the foundations of our undertaking in the following manner. By creation we mean only creation of nebulæ.

We shall suppose all space—if we can comprehend what that means—to have been filled with the ether, and the law of attraction to have been in force previous to the time when our operations are supposed to have commenced. These we may consider to have been the first acts of creation, or to have existed from all eternity. Then, in that part of space occupied by our universe—even though it should extend infinitely beyond the reach of our most powerful telescopes—we shall suppose the work of creation to have begun by filling the whole of that space with what are known as the chemical elements, reduced to their atomic state. We do not want to have molecules or particles of matter, or meteorites or meteors ; because they involve the idea of previous manipulation or agglomeration, but matter in its very simplest form, if there is any more simple than the atomic. At this stage the most natural idea is to suppose that the whole of this matter was at rest, without motion of any kind, because we cannot understand how motion could be an object of creation, but can very easily see how it might be of evolution ; and because, under the law of attraction, matter had the elements of motion in itself. Under that law it is quite possible for us to comprehend that all the suns of our universe could have been formed just as they are, with all their movements of rotation, revolution in the cases of multiple stars, and translation or what is called proper motion. And it is within the bounds of possibility that future astronomers may be able to show how these movements have been brought about, should it ever be possible for them to find out and define with sufficient accuracy what the translatory, or proper, motions are. Then, as for the temperature of this newly created matter, we have no resource left but to suppose that it must have been that of space, whatever that may have been then, even as we have been obliged to say before.

Once created, the atoms of the cosmic matter would immediately begin to attract each other in all directions, and form themselves into groups. At first thought it might be supposed that these groups, and suns formed from them, ought to have been all of the same size, being formed from the same material under the same conditions, but nature, or evolution, seems never to be disposed to produce the same results in its manipulations of matter, whatever they may be. When the water is drawn off from a pond, and the mud left in the bottom of it allowed to dry in the sun, it breaks up into cakes of very various shapes and sizes. No doubt there are physical causes for this being the case, but, though perhaps not altogether impossible, it would be a hard task to find them out. Much more so would it be with originally created matter, and we have only to accept the fact. Moreover, there can be little doubt but that the universe was formed, evolved, according to some design—not at hap-hazard—and that the cosmic matter was created with the distribution necessary to carry out the plan. That the stars differ from each other in magnitude is the best proof of design; for no one can believe that inert matter could determine into what shapes and sizes it could arrange itself. But we have now nothing more to do with the universe, and will confine our operations to the domains of the sun.

Notwithstanding the vagueness and dimness of the description we have been able to give of the part of space to which our work is now to be confined, we can conceive it to resemble in some degree—not a comparatively flat but—a round starfish, with arms more unequal in length, and irregular in position than the kind we are accustomed to see. In such an allotment of space we can easily conceive that the work of attraction and condensation, of the newly created cosmic matter, in forming itself into a nebula, would be most active in the main body; that in the arms, or projecting peaks as they may be called, it would go on more slowly in the direction towards the centre, the quantity being smaller; and that on account of the greater distance in each from the centre of attraction, and of its being more under the influence of the still existing counter-attraction of the matter in the domains of the sun's neighbours, they might become almost, or rather altogether, detached from the more rapidly contracting main body.

We shall, then, suppose that all this has taken place in our incipient nebula. The centre of attraction would at first be the centre of gravity of the whole region occupied by the cosmic matter, which would be ruled in due measure by the projecting peaks, and the indentations or hollows produced in it by the attractive force of the most powerful neighbours; which hollows would gradually disappear as the process of condensation went on, and the main mass would assume the figure of a nebula of some shape. From this stage we may reasonably conclude that, as it was contracting towards the common centre of gravity of all its parts, it would gradually assume a somewhat globular form, and we may now suppose it to have contracted to, say three times the diameter of the orbit of Neptune. Here, then, we may take into consideration what was the interior construction of the main mass which we may now look upon as a nebula; and we have only two states in which we can conceive it to have been. Either that the whole was condensing to the common centre of gravity, in which case its greatest density would be at the centre; or that it was condensing towards the region of greatest mass, in which case its greatest density would be at that region, and its least density at the exterior of the nebula, and also at, or at some distance from, its centre; that is, that the nebula was hollow and without any cosmic matter at all at its centre.

In the first case we must recognise that, from that period of time at least, the cosmic matter that was at, or even near, the centre of gravity then, must be there still all but inert, and being gradually compressed to a greater and greater degree of density. There would, no doubt, be attraction and collisions going on amongst the particles, with condensation towards the centre and production of heat—as long as the particles retained the gasiform condition—which might be increased in activity by the pressure, or superincumbent weight, of the whole exterior mass, but there would be no tendency in them to move outwards—provided their gravitation was always towards the centre ; and any motion amongst them would be of the same kind as the vibration of the particles of air shut up in a cylinder and gradually compressed by a piston forced in upon them, and not allowed to escape owing to the sides of the cylinder exerting upon them a pressure increasing exactly in the same proportion as the pressure on the piston was increased. And if this was the case with the matter at or near the centre, it would be the same with that of the whole mass, with the exception, perhaps, of the outer layer, which might act the part of the piston in the cylinder. There could be no motions among the particles, except those of collisions and of falling down towards the centre. The outward impacts of collisions would be less strong than those inwards, on account of gravitation acting against them, and the general tendency of all matter would be to move towards the centre. Even were we to assume that the whole mass was endowed with a rotary motion, the result would be much the same, that is, increasing stagnation of the matter as it approached to the centre. The areolar law teaches us, however, that the increase of condensation at the centre would increase the rotation there ; but in that case we have to acknowledge that this increase of rotation would have to be propagated from near the centre to the circumference, which would be by far the most difficult mode of propagation, and we are forced to think of what would be the rate of rotation at the centre, of a nebulous globe, of some sixteen thousand million miles in diameter, required to produce a rotation at the circumference of even once in four or five hundred years ; and from that to think of what must be the speed of rotation at the centre of the sun, at the present day, to produce one rotation at the circumference of twenty-five to twenty-seven days. We should also have to think seriously of how the rotary motion was instituted, and we could only appeal either to simple assumption, or to the impact theory, which, applied to a mass of the dimensions of the one we are dealing with, would require more explanation than the whole formation of the nebula itself.

In the second case, that is, looking upon the nebula as a hollow sphere—when it was of the dimensions we have just supposed it to be—we get rid of all the difficulties, and we may add impossibilities, that we encountered in the first case. In such a formation there could be no particle of matter in a state approaching to inertness, not one that could not work its way, through force of attraction and collisions, from the outer to the inner surface of the hollow shell, or *vice versâ*, or all through and round it and from pole to pole—if it had poles then; it might increase or decrease in density, according to the density of the particles with which it came into collision, as it moved from one place to another, but it would find no spot where it could stand still or be imprisoned. Even arrived at the region of greatest density, it could change places with its neighbours and move all over that region, if it were condemned to remain with one density once it had acquired it; if not, by acquiring or loosing a little density—*i.e.* by being compressed or allowed to expand a little—it could work its way outwards or inwards, as we have just said, and be as free as the law of attraction would admit of, and as active as that law would oblige it to be. It must be borne in mind that gravitation would act in two opposite directions depending on whether it was acting on the outside or inside of the region of greatest density. We do not go the length of supposing that it could escape altogether from the nebula were its progress outwards; because, as it approached the border, it would meet with plenty of other particles coming in, which would reduce its velocity and prevent its escape. Besides, the law of attraction would take good care to prevent it from passing over to a neighbour nebula or sun.

It may be argued that in the first case—*i.e.* condensation to the centre—the particles would have the same facilities for changing place, in so far as moving all round the interior of the nebula, or across it, on their way to quasi stagnation, as their densities and the superincumbent weight concentrated and increased; but there could be no motion outwards because the *attraction of gravitation* would not permit it; nothing could *fall upwards*, all must *gravitate* to the centre. Thus the power of motion in the particles would be limited to very much less than half what they would have in the case of the hollow sphere.

It will not do to argue that the increasing heat at the centre would create an upward current. It might create repulsion and prevent the farther-out particles from so soon reaching their final resting or vibrating place, but it could not create an upward convection current of any magnitude; because the colder particles falling down to replace those rising up—that is, if the warmer ones did rise up—being greater in number because occupying greater space, would soon cool down the centre and put an end to the upward current, that is, if it ever came to be set in motion. The greater weight of the greater number would be sure to keep the lesser number in their prison. If any one should say that those nearest the centre would be the heaviest, let him remember that the heaviest liquid or fluid does not rise to the surface. There could be no furnace at the centre to heat the cold particles as they came down to replace those that had just risen up; and if there was, it would be gradually cooled and extinguished. In fact, the centre region would become colder than that immediately outside of it, and so on until the greatest heat would be at the surface of the nebula. Should it be argued that the vastly greater number of particles in the outer regions would help those at the centre to rise up, we agree; but it would be because the attraction would be greater outwards than inwards, as we have shown all along, and not because the pressure forced them out—against itself. But, it must be added, this means that if there was still a plenum at the centre the particles that had once left the centre could never come back again, nor any others to replace them, and that no convection current could ever be formed for carrying heat or matter from the centre, or its immediate neighbourhood, outwards.

In view of the above comparison of the two cases—added as a complement to what we believe we have demonstrated in a former part of our work—we shall adopt the second as being most in harmony with the laws of attraction, and of nature in general, and shall endeavour to describe in some detail, the construction of the nebula out of the matter belonging to the domains of the sun, as we have marked them out.

We have already said that on account of being at the greatest distance from the main body, and at the same time nearer than all other parts of it, to the attractive force in the domains of the neighbouring stars or nebulæ—which attraction continues to be exerted upon the solar system up to the present day—the matter in the high peaks which we have shown would form part of the sun's domains, would come to be completely separated from the rest of the nebulous matter. We shall now assume this to have come about, the detached pieces, somewhat in the shape of cones, occupying positions distant from the main body, in some sort of proportion to their altitudes and masses. This separation would naturally make some alteration on the centre of gravity of the remaining mass. It would come to be nearer to the deep hollows, made in the mass by the attraction of the most powerful of the nearest neighbouring stars; and as we have seen that the hollows made by Sirius and *α* Centauri would be the deepest, and also for greater simplicity in description, we shall suppose that the centre of gravity would come to be nearer to these hollows than it had been before. Then, as the condensation and contraction proceeded, the tendency would be to fill up these hollows, and, as a consequence, the matter at the opposite side of the nebula would at the same time tend to lag behind in approaching the centre—for the same reasons we have given in the case of the peaks—and might easily come to be detached from the main body altogether, first in the form of shreds, then in larger masses, and afterwards in concave segments of hollow spheres, as contraction advanced; and the whole seen from a sufficient distance, would have the appearance of a nebula with crescents, perhaps almost rings, of nebulous matter and detached masses on one side of it; all very much like what we know to be the figures presented by some nebulæ.

When contraction had continued till the hollows caused by Sirius and *a* Centauri were filled up, we might suppose that the nebula had come to be somewhat of a spherical form, although far from being very pronounced, and we have now to consider what its internal structure might be and most probably was.

In describing the construction of the earth-nebula we showed that particles of matter placed at different parts of its interior, even not very far from the surface, would be drawn out, in the first place by the greater number coming in from a greater distance from the centre, and that when they met they would all be drawn in towards the centre by the conjoint attraction of the whole mass; and now we can apply this fact to the larger solar nebula, and consider what might be the result. Let us fix upon a certain number of equidistant zones in a sphere of cosmic matter, extending from the centre at *a* to *b*, *c*, *d* and *e*, at the surface. We know that, according to our former reasoning on particles, and the law of attraction, part of the matter of the zone at *a* will be drawn outwards by that at *b*, while part of that at *b* will be drawn inwards by that at *a*, and that the same will take place with all the other zones out to the surface at *e*; and thus there might come to be congested layers between these equidistant places, and there might even be formed hollow spheres within hollow spheres, independent of each other, all through the nebula from near the centre to the surface. This idea is by no means fanciful, as is witnessed by the accounts given in Chambers's "Handbook of Astronomy," already referred to, Vol. I., and the Figs. 215, 222 and 223, showing the form and appearance of the remarkable comets of 1874 and 1882. If different, almost concentric, zones or layers of cosmic matter can be constituted in the hemisphere forming the head of a comet, there is no reason why concentric layers of the same matter should not be formed in a nearly spherical nebula. In fact, we can appeal to what is seen in the heads of the two comets cited, Donati's also represented in the same work, Figs. 199-203, as convincing proof of the correctness of our contention and demonstration that all satellites, planets, suns, and stars are hollow bodies. Even the tails of comets, at least of the larger ones, are acknowledged to be hollow bodies.

When steadily looked into we find the notion that all fluid bodies are hollow to be much more common than is perhaps generally believed. Beginning with the smallest, we find what follows in the Rev. Dr. Samuel Kinn's work, entitled "Moses and Geology," Edition 1889, page 86: "A mist, whether in the form of a cloud or fog, is composed of small bodies of water obeying the laws of universal gravitation by forming themselves into spherules, which Halley and other eminent philosophers thought to be hollow. As water is heavier than air, scientists were for a long time seeking for a good reason to account for clouds floating. It may be that Kratzenstein has somewhat solved the problem. He was examining in the sunshine some of the vesicles of steam through a magnifying glass when he observed upon their surface coloured rings like those of soap-bubbles, and some of the rays of light were reflected by the outside surface, others penetrated through and were reflected by the inner surface; he concluded, therefore, that the envelope of the sphere must be excessively thin to admit of this taking place. We may, therefore, suppose that these vesicles are filled in some way with rarefied air, and are so many little balloons whose height in the atmosphere varies in proportion to the density of the air they contain. How this enclosed air should become rarefied on the formation of the tiny globule is a problem still to be solved."

Dr. Kinn says nothing of *how* the spherules of cloud or fog were formed by the laws of universal gravitation, nor *why* Halley and the other eminent philosophers thought them to be hollow, and only states the fact that Kratzenstein found the vesicles of steam to be hollow; and only one cause can be assigned for such being the case, namely, the manner in which we have shown how hollow spheres can alone be formed. That the vesicles of steam examined in the sunshine were hollow it would seem there can be no doubt; and if so, there can be as little that Halley and the others were right in thinking the spherules of clouds to be hollow. The steam vesicles could not come into existence at once in the air, in form large enough to be examined through a magnifying glass, but must have been built up out of a multitude of the very smallest atoms of water turned into vapour; and would follow the same law as the atoms of cosmic matter and so form the little balloons. In their formation the hollow space would be filled with air, which would expand when heated and contract when cooled, and so regulate their height in the atmosphere. And thus the problem of the last sentence of the quotation is solved.

We shall now go to the opposite extreme of matter, and see what Mr. Proctor says when treating of the formation of a Stellar System; but we must state that it is not very clear to us, whether he is exposing Mädler's ideas or his own, although we think they are his own or, at least, adopted. He says in "The Universe of Stars" at page 112: "He (Mädler) argues that if a galaxy has a centre within the range of the visible stars, a certain peculiarity must mark the motions of the stars which lie nearer to the centre than our sun does. As has already been mentioned, the neighbourhood of the centre of a stellar system is a scene of comparative rest. In the solar system we see the planets travelling faster and faster, the nearer they are to the great ruling centre of the scheme; and the reason is obvious. *a.* The nearer a body is to a great centre of attraction like the sun, the greater is the attraction to which it is subject, and the more rapid must its motion be to enable it to maintain itself, so to speak, against the increased attraction; but in a vast scheme of stars tolerably uniform in magnitude and distribution, *the outside of the scheme is the region of greatest attraction, for there the mass of all the stars is operative in one general direction.* (The italics are ours.) As we leave the outskirts of the scheme, the attraction towards the centre becomes counterbalanced by the attractions towards the circumference; and at the centre there is a perfect balance of force, so that a body placed there would remain in absolute rest. It is clear, then, that the nearer a body is to the centre, the more slowly will it move." (Compare this last sentence with the one beginning at *a* above.)

Here we have recognised, the principle that in a star system the immensely greater number of stars at the outside of the scheme would produce a perfect balance of force, and that a body placed at the centre would remain in absolute rest. This agrees wonderfully well with what we have been arguing, a few pages back, with respect to a sun solid to the centre. Matter at the centre would be at absolute rest, *dead*, that nearest to it would be nearest to dead, and so on through a sun or planet, gradually coming to life as it came nearer to the surface ; exactly as we have shown it would be, having in it little more than rotary motion. When once acknowledging the immense superiority of attractive force of the stars at the outskirts of the system, over the very few there could be at its centre, Mr. Proctor seems to have stopped short with the idea and to have contented himself with one body at the centre in absolute rest. Had he gone one step further he must have seen that one, or even a very few, could not maintain themselves near the centre with such an immense number pulling them away in every direction. There could be no perfect balance of force. And had he applied the same idea to the earth, and followed it out to the end, he could not have written as he has done, in "The Poetry of Astronomy," at page 354, " that the frame of the earth is demonstrably not the hollow shell formerly imagined, but even denser at its core than near the surface." He would have found some difficulty in fixing his first dead particle at the centre, when there were such infinite hosts of near and far-off neighbours endeavouring to annex it. He would have found that the absolute rest was neither more nor less than absolute vacuum. It is utterly impossible to show how any body could be built up out of a nebula of cosmic matter, or even meteorites, from a solid centre, under the law of attraction. We repeat that any foundation laid there would be in a state of unstable equilibrium, and would be hauled away out of its place never to return; unless the cosmic matter around it were so perfectly arranged on all sides that its attraction on the foundation would be absolutely equal in all directions; a condition which cannot be imagined by any one who takes the trouble to think of it. And we think we may add, that no body could be established at the centre of a system of any kind unless it were of sufficient magnitude to control the whole matter within range of it, exactly as we see in the solar system; and that the central body could be no other than a hollow sphere. Thus we have either to look upon the sun with his planets and their satellites as hollow bodies or to conclude that the solar system was not formed out of a nebula.

Coming back to our nebula after the hollows in it, caused by the attraction of Sirius and *a* Centauri, were filled up, and when we showed that it might have had the interior form of a series of hollow spheres one within the other, and also might be accompanied by crescents and shreds of cosmic matter on the opposite side to the hollows—a supposition we put forward more in explanation of what is to be seen in some nebulæ and comets, than as in any way necessary for our purposes—then, even although it had been separated interiorly into different layers or concentric shells of spheres, these layers continuing to attract each other, would finally come to form one hollow sphere with its greatest density at the region where the inwards and outwards attractions came to balance each other. Long previous to this stage—even from the very beginning—the atoms gradually coalescing into larger bodies, would be attracting, colliding with, repelling and revolving around each other, sometimes increasing in dimensions, at others knocking each other to atoms again; but there would be a tendency in them to combine into larger masses as they approached the region of greater density, where the attraction was greatest.

Now, if the collisions and encounters amongst the masses, great and small, always exactly balanced each other, the whole mass of the nebula would gradually contract towards the region of greatest density, and the whole would ever remain without any other kind of motion in it than what can be seen in a mity cheese—a kind of congeries of particles heaving in every, and at the same time in no, direction. But as an absolute balance of collisions could not be maintained for ever, especially where they would be constantly varying in force and direction, a time would come when movements of translation, as well as of collision, would be instituted on a large scale, in many directions, which, if they also did not manage to balance each other—an affair equally as impossible as in the other case—would ultimately resolve themselves into motion in one predominating direction through the whole nebula.

We do not forget that we are dealing with the shell of a hollow sphere, not with a ring, or section of a cylinder, and we can conceive that there would be, from the first, partial motions of translation in multitudes of directions, radial, angular, transverse, etc. etc., constantly changing, even being sometimes reversed, but also constantly combining with each other, and gradually leading on to decided, though partial, uniformity in one direction. As a matter of course this motion of translation would be controlled by its own constituent parts attracting each other to some extent, and thus a rotary motion would be established in the interior of the nebula in the region of greatest density. We can also conceive that when the motions of translation had become nearly uniform, the plane of that uniform motion might be in any direction through the whole mass of the nebula, and might be continually varying until final uniformity was attained, when the greater part of the mass was moving in combination, and the rotation was thereby firmly established in one direction, though still not embracing the whole.

We have to take into account also that when the rotary movement had settled down into one plane, it would be most active at the distance of the region of greatest density of the nebula from its centre; in fact it would be instituted at that region and be, therefore, most active there; and then the most active part of the matter would be in the form of a rotating ring, still surrounded by an immense mass of nebulous matter, both inwards and outwards, to which it would gradually communicate its own motion, until the whole mass would rotate, in one direction, on an axis. But it is evident that in the whole rotating mass there would be different degrees of velocity of rotation at different places, decreasing from the supposed ring inwards towards the centre, and outwards to the surface at what would thus become the equatorial region ; and also decreasing from the equatorial plane to the poles. Following up this idea, we have a more reasonable manner of accounting for the different velocities of rotation observed on the surface of the sun, between the equator and the poles, than we have seen suggested in any speculations on the cause that have come under our observation. Until rotation was fully instituted, the areolar law could have no power over the multitudinous movements going on in the nebula, but from that time it would begin to act, and condensation would increase it at the region where it began ; and as all increase had to be propagated from there, inwards, outwards, and in all directions, the differences in velocity of rotation throughout the sun must endure as long as he continues to contract. In this we find an immense field for producing heat in the sun, from the eternal churning which must be going on in the interior.

A rotary motion produced in this way might have two different results : in one case the rotation might be continued until the matter at the polar regions had all fallen in towards the centre, and had been thrown out afterwards by centrifugal force and the whole mass converted into a nebular ring, in the form of the annular nebula in Lyra. In the other case we could conceive that, in a smaller nebula, the centrifugal force of rotation caused zones to be abandoned at the equatorial surface, in the manner set forth by Laplace in his hypothesis, and that the matter from the polar regions fell in more or less rapidly for the formation of the different members of a system like the sun's ; and that the dimensions of the planets would be determined by the rapidity with which the matter fell in as the process went on. Such a conception would help to account for the outer planets of the solar system being so much larger than the inner ones, because there would be more matter falling in ; and make us think that the nebula in Lyra is destined to form a system of multiple stars.

Some years after this mode of instituting rotary motion in a nebula was thought and written out, and also an extension of it to which we may refer later on, we came upon a kind of confirmation of the correctness of our views in an article in "Science Gossip" of January 1890, on the nebular hypothesis, where it is said: "We have established, then, the existence of irregular nebulæ which are variable—that is, the various parts of which are in motion. . . . . Now, with the parts of the nebula in motion, whether the motion is in the form of currents determined hither and thither according to local circumstances, or in any other conceivable way, such motions bearing some reference to a common centre, unless the currents exactly balanced each other—a supposition against which the chances are as infinity to one—one set must eventually prevail over the other, and the mass must at last inevitably assume the form peculiar to rotating bodies in which the particles move freely upon each other. It must have become an oblate spheroid flattened at the poles and bulging at the equator, rotating faster and faster as it contracted. In some such manner has our solar system acquired its definite rotation from west to east."

The writer in "Science Gossip" has taken the irregular motions in the nebula as made to his hand, and has come to the same conclusion as we have, namely, that they would all resolve themselves into motion in one direction only, always subject to the general attraction towards the centre of gravity of the nebula, which means motion round a centre, perhaps not necessarily rotary motion. However, the only difference between his ideas and ours is that we deal with a hollow nebular shell, in which, it will be acknowledged, it would be much more easy for the law of attraction to produce marked and distinct motions of any kind, and which would lead to one motion in one direction throughout, than in a nebula homogeneous, or nearly so, from the surface to the centre. Whether it would lead to the formation of an oblate spheroid is another question, as that might depend on a variety of circumstances, one or more of which we shall have to touch later on; in fact, we have already shown how the very reverse might be the case.

CHAPTER XVI.

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300 Repetition that a spherical body could not be made from a lens-shaped nebula by attraction and condensation.

TESTING THE PRACTICABILITY OF THE HOLLOW SPHERE THEORY. RETROGRADE MOTIONS, POSITIONS, DENSITIES, MASSES, ETC. ETC., CONSIDERED.

BEFORE going any farther it will be convenient to try to find out whether the solar system could have been constructed from a hollow nebula such as we have been describing gradually contracting as the matter for the formation of one planet after another was abandoned until—as we have put it —the nebula could abandon no more matter, and finally resolved itself into the sun. For this purpose we may suppose it to have been condensed and contracted until its extreme diameter was 6,600,000,000 miles; the same as we supposed it to have been, when we began the analysis of the nebular hypothesis. We will not now, however, suppose it then to have contained the whole of the cosmic matter out of which the system was formed, as we did before ; because we have seen as we have come along that a very considerable part of that matter must have been left behind, almost from the moment that contraction commenced. We have already given the reasons for this in describing the domains of the sun ; and, leaving the peaks out of account altogether for the present, we will only deal with the regions of what we have called the main body.

Although we have fixed a limit beyond which the neighbouring stars could not draw off any cosmic matter from the domains of the sun, that does not mean to say that their attractive powers would cease at that limit ; because we have had to acknowledge that each one of them continues, even now, to exert its attractive power up to the very centre of the sun. They would still have power to counteract, in some measure, the sun's attraction of the matter of the nebula towards his centre, and the result would follow that there would be one or more, even many, fragments of the main body which would be left more or less behind, and in varied forms, when the more central part had contracted to the dimensions to which we have now reduced the nebula—all much the same as we have already said a few pages back.

When the nebula was 6,600,000,000 miles in diameter its volume would be $150{,}533^{24}$ cubic miles—as we have seen at page 87—the half of which is $75{,}266^{24}$ cubic miles, corresponding to a diameter of 5,238,332,000 miles, or radius of 2,619,166,000 miles. Now, according to our theory, it would be at this distance from the centre that the greatest density and activity of the nebulous matter would be, where we have just been showing how a movement of rotation could be generated, and where, in consequence, its motive power, so to speak, originated and existed. Here we find by dividing 5,238,332,000 by 6,600,000,000 that the region of greatest density in such a nebula would be at 0·7937 of its diameter. In our calculations about the earth, as it is, the proportion was found to be 0·7939, but the densities of the outer layers were empirically arranged by us; and, besides, almost the whole of the mass was supposed to be solid matter, so that no accurate result could be expected from that operation. There also we found that the inner surface of the hollow shell was at 0·5479 of the whole diameter, which we may adopt for the nebula we are about to deal with, as that dimension may be varied considerably—so may the other also—without in any way vitiating our theory.

Having found these proportions, which can only be considered as distantly approximate, let us go back to the 9 nebulæ—excluding the final solar one—into which we supposed the original nebula to have been divided—in the analysis just alluded to—and see how the regions of greatest density in them would correspond to the orbits of the planets formed out of them. This examination requires a good deal of calculation and accompanying description, which it might be found tiresome to follow, and would really answer no good end were it written out; so we shall suppose it to be made and the results obtained from the calculations to be represented in the form of Table IX., where they can be seen at a glance almost, and compared without much trouble. This arrangement will also furnish a readier means of reference for the remarks we shall have to make on, and the information obtained from, the examination. And we have still to add that the extreme diameters of the 9 nebulæ are the same as those we used for the analysis; as also, that we make use of only the first of the proportions just cited, viz., 0·7937, it being the only one required for determining the positions of the regions of greatest density in the nebulæ.

From the table we see that the region of greatest density of our original nebula was at 6·26 per cent. *within* the distance of Neptune's orbit from the sun, a state of matters which precludes the idea of condensation during, at least, a great part of the act of abandoning the ring for the formation of that planet. But it will be remembered that we gave it the diameter of 6,600,000,000 miles without assigning any adequate reason for doing so, and, we can say with truth, with the

TABLE IX.—DIMENSIONS OF THE NINE NEBULÆ, WITH THEIR DIAMETERS AND REGIONS OF GREATEST DENSITY COMPARED WITH THE DIAMETERS OF THE ORBITS OF THE PLANETS FORMED FROM THEM.

Name of Planet.	Nebula.	Region of greatest Density.		Orbit of Planet.		Region of greatest Density compared with Orbit.		
	Outer Diameter in Miles.	Diameter in Miles.	Radius in Miles.	Diameter in Miles.	Radius in Miles.	Within, in Miles.	Without, in Miles.	Per cent.
Neptune .	6,600,000,000	5,238,332,000	2,619,166,000	5,588,000,000	2,794,000,000	174,734,000	..	6·26
Uranus .	4,580,000,000	3,635,146,000	1,817,573,000	3,566,766,000	1,783,383,000	..	34,190,000	1·92
Saturn . .	2,672,000,000	2,120,766,400	1,060,383,200	1,773,558,000	886,779,000	..	173,604,200	19·58
Jupiter .	1,370,800,000	1,088,003,960	544,001,480	967,356,000	483,678,000	..	60,323,480	12·47
Asteroids .	744,000,000	590,512,800	295,256,400	520,600,000	260,000,000	..	35,256,400	13·56
Mars . .	402,000,000	319,067,400	159,533,700	283,300,000	141,650,000	..	17,883,700	12·63
Earth . .	234,620,000	186,217,894	93,103,947	185,930,000	92,965,000	..	138,947	0·15
Venus . .	160,210,000	127,158,677	63,579,339	134,490,000	67,245,000	3,665,660	..	5·45
Mercury .	103,230,000	81,933,651	40,966,825	71,974,000	35,987,000	..	4,979,825	13·84
	Had the position of Neptune been normal, the above data for him and Uranus would have been as under. More or less.							
Neptune .	8,299,785,830	6,587,540,800	3,293,270,000	5,588,000,000	2,794,000,000	..	499,270,000	17·86
Uranus .	5,144,439,610	4,083,042,000	2,041,521,000	3,566,766,000	1,783,383,000	..	258,138,800	14·48

idea, more than anything else, of not increasing the almost unimaginable tenuity of the matter composing the nebula; and the position of Neptune in the system is so peculiar compared with the other planets, that it cannot be properly used as a standard for any kind of inquiry. The result obtained above can therefore be of no use for the investigation we have undertaken. Not only so, but the almost similar result in the case of Uranus is also rendered useless from the same cause, in which we find that the region of greatest density of the nebula is only 1·92 per cent. beyond the orbit of the planet. If the mean distance from the sun of Neptune's orbit had been what was used by Leverrier in the calculations which led to his discovery, namely, 36·152 radii of the earth's orbit, the region of greatest density of the Uranian nebula would have been 14·48 per cent. beyond his orbit, as may be seen from the addition to Table IX., in finding which we have used the same system as in all our work.

In the next four nebulæ of the table—including the one we introduced to represent the Asteroids—we see that their regions of greatest density are respectively 19·58, 12·47, 13·56 and 12·63 per cent. farther out from the centre of the sun than the orbits of the planets formed from them. Here, then, we see a very apparent approach of uniformity, and can say with much reason that planets could certainly be formed out of the matter abandoned, through centrifugal force, by hollow nebulæ similar in construction to what we have demonstrated that of the original nebula to have been; each of them occupying the position corresponding to its orbit.

Following these come the Earth and Venus nebulæ. In the former, the region of greatest density almost coincides with the orbit of the planet, being only 0·15 per cent. beyond it, instead of something like 12 per cent. as it ought to be to conform with the four preceding cases; and in the latter it is 5·25 per cent. within the orbit of the planet to be made from it. But in this case we have to note that the orbit of Venus is 3·33 per cent. beyond the position pointed out for it by Bode's law, and that it is the only one of the whole number of planets whose orbit is farther removed from the sun than the distance assigned to it by that law. Also we see from our reversal of Bode's law, that the rates of acceleration of rotation for these two planets are 1·880 for the earth and 1·626 for Venus, instead of the average of 2·5896 of the four preceding planets; that the density of Venus is less than that of the Earth, instead of being greater as it is successively in all the other planets from Saturn inwards; and we may add that the diameters are nearly equal. All showing that influences had been at work in the formation of these two planets, different to those in the preceding four; and that until we know what these influences have been, we cannot account for any anomalies produced by them. Neither are we called upon to consider that our theory is destroyed by these anomalies, any more than it can be by the anomaly in the case of Neptune's position.

Lastly, we have in Mercury the region of greatest density of his nebula at 13·55 per cent. beyond his orbit, and the rate of acceleration of revolution over Venus 2·5543 times, both of which conform fairly well with the same noted facts, in relation to Mars, the Asteroids, Jupiter, Saturn, and, we may add, Uranus. But, in justice, we must not omit to add that there may be some error in the excess of 13·55 per cent. in the distance from the sun beyond his (Mercury's) orbit, arising from the fact that there may have been some difference from what we made it to be, in the line of separation between his nebula and that of Venus; and also that we had to guess at the line of separation between his and the residuary nebula. Moreover, it has to be taken into account that his orbit is 3·22 per cent. within the position assigned to it by Bode's law.

From the Table IX., and an examination of it, we learn that out of the 9 nebulæ into which we divided the original one, in the analysis of the nebular hypothesis, we have five—four of which are consecutive—which may have been almost of the same construction, and not far from the same proportions; that the original nebula cannot, for reasons assigned, be looked upon as either similar, or the reverse, to the five just classed; that one, the Uranian, is practically similar to the five, and might be exactly similar could the anomaly in the position of Neptune be explained ; and that the remaining two, the Earth and Venus nebulæ, seem to show that they have been abandoned in a manner different from the others. Perhaps we may be able, later on, and in a different way, to give a reasonable explanation of the anomalies in the positions occupied by Neptune, the Earth, and Venus, and also of the peculiarities of their dimensions. So far, we believe we are justified in concluding that out of the 9 nebulæ, 6 may really be considered as supporting our theory, and the remaining 3 as, in all probability, capable of being shown to be, at least, not opposed to it. To this we may add that on several occasions we have stated our opinion, that the divisions between the nebulæ we have established, could not have taken place at the half-distance between the orbits of any two planets, but much nearer to the outer one. It is evident, then, that if we had made the divisions at any distance farther out, say at three-fourths of that distance from the inner orbit, the extreme diameter of each one of the nebulæ would have been just so much greater, the region of greatest density farther out from the centre of the sun, and even that of Neptune would have been beyond his orbit. All this could be done, yet but it would serve no good purpose, as will be seen presently ; and we might be accused of cooking our data in order to produce a result favourable to our theory.

We have made the foregoing examination because, when we began our work, the general idea was that, according to the nebular hypothesis, the material for the formation of each planet was abandoned by the ideal nebula in a distinct and separate mass from any other—we are not at all sure, however, that this was Laplace's idea. This, we found out, could not be the case when we attempted to give some sort of separate or distinct form to the matter out of which Neptune was supposed to have been formed ; and when we became convinced that all the matter abandoned by the nebula, from first to last, must have been thrown off in one continuous and, most probably, uninterrupted sheet. This, of course, makes us think of how the division of the sheet into separate rings was brought about, for there must have been absolute separation between them, otherwise separate planets could not have been made out of the sheet; and the only explanation that can be given is, that it must have depended on the quantity of matter that was abandoned, in nearly equal times, at different periods of the operation; for the areolar law precludes the idea of there having been very rapid changes in the rate of rotation of the nebula, and certainly of its decrease at any period as long as condensation and contraction went on. Whereas, although the sheet thrown off may have been continuous, we have no reason to suppose that it was of constant volume or density from beginning to end of the operation; in fact, we have already seen that its density was constantly increasing, and have suggested, in the reversal of Bode's law, that the differences in dimensions and densities of the planets have arisen, from irregularity in the quantities of matter abandoned from time to time. This irregularity could only arise from the mode of construction of the nebula, and from the forms it assumed during condensation, as we shall attempt to show in due time. Meanwhile we can conclude that the region of greatest density in any of our nebulæ had no influence whatever on the position of the orbit of the planet that was formed out of it.

We have shown, very clearly we believe, at page 109, from quotations—at second hand—from his own exposition of his hypothesis, that Laplace considered that condensation could only take place at the surface, or in the atmosphere as he called it, of his nebula, on account of its being possible only after radiation into space of part of its excessive heat; and that consequently there could be no acceleration of rotation in the nebula, due to the areolar law, except where there was condensation. On the other hand, in our cold hollow-sphere nebula, condensation could only take place at the region of greatest density, or greatest mass, which must be always very much nearer to the surface than to the centre; so that in both cases, equally, the abandoning of matter under the influence of centrifugal force would be virtually the same, and no further remarks are called for, on our part, on that head.

Neither is it necessary for us to show how planets could be formed out of the rings abandoned by their respective nebulæ, for everybody seems to agree that when they broke up, the fragments could not do otherwise than form themselves into small nebulæ, which in the course of time condensed into planets. M. Faye's explanations are good for that.

With respect to their motions of rotation being direct or retrograde, we have seen, at page 116, and following, that Laplace's description of how the former motion could be brought about is mechanically correct ; and, at page 121, that he did not consider that the direction of revolution of a ring necessarily demands that the rotation of a planet formed from it should be in the same direction. As already said, he has shown how direct rotation could be produced, and we have no doubt that he could have shown how retrograde rotation could also be produced, had he found it to be at all necessary. Be that as it may, however, it is a very simple matter to show how, following our method of construction of the primitive nebula, the retrograde rotation of Uranus and Neptune could, or rather must, have been determined.

It will be remembered that when we were "getting up" the original nebula in the domains of the sun, whose form we described as well as our limited means would admit of, we said that when the cosmic matter contained in them began to contract, not only the parts contained in the peaks and promontories would soon be left behind, and come in at a slower rate, but also large masses of the outer part of the main body, especially of what was on the sides opposite to the deep hollows made in the domains by the most powerful of the sun's neighbours, in the form of fragments, crescents, and parts of hollow segments. Let us now, then, suppose the operation of planet-making to have advanced so far that the whole nebula was rotating on its axis, and abandoning matter through centrifugal force, from its equatorial regions in a continuous sheet, as we have said several times that it must have done, and that the matter destined for Neptune and Uranus has not only been abandoned, but divided into two distinct rings—a supposition made in this case only for facility of description. Then some of the matter which had been left behind, but still being gradually drawn in, would be almost totally intercepted in the equatorial regions of the nebula by these two rings, and would fall in greater quantity upon their outer edges than anywhere else, more especially in the case of the outer one. These adventitious additions would come in without any angular, or tangential, movement whatever, because rotary motion was not yet established in them, and would retard the revolutionary movement of the rings—in decreasing degree from their outer to their inner edges—while acquiring angular motion themselves ; and would also intensify the original difference in revolutionary motion already existing at these edges. At the same time these additions of extraneous matter would seriously impede the contraction of the rings in the radial direction on account of their volume, but would have little or no effect on contraction in the circumferential direction ; the consequence of which would be that they would break up before friction, and the mutual collisions of their particles, had time to produce a uniform revolving motion throughout their whole breadth ; that is, while their inner edges would be still revolving with more rapid velocities than the outer ones ; and the rotary motions of the planets derived from them would be retrograde, according to M. Faye's demonstration—or that of any other who has taken the trouble to think over the matter. And we may add that this mode of reasoning, applied with a little more detail, will very fully account for the rotation of Neptune being more decidedly retrograde than that of Uranus, because the quantity of matter so deposited on the outer flat ring in this process would unquestionably be greater than on the inner one, and consequently the difference of velocity between the outer and inner edges of the two rings also greatest on the outer one.

We take it to be unnecessary even to say that, the revolution of the satellites of these two planets being retrograde and anomalous, the rotation of their principals must be retrograde and anomalous also.

Before going any farther we have something to say about the anomalous position of the orbit of Neptune, which is certainly not the position sought for by M. Leverrier ; in fact, the elements employed by him in his calculations to discover a perturbing planet—whose existence may be said to have been known—are so different from the elements of the one actually discovered, that there would be nothing out of reason in saying that Neptune is not the perturber that was sought for, but only an instalment of the perturbing force. It may raise a storm in some quarters to say so, but the fact remains the same, or it must be confessed that mathematics is a more elastic science than it professes to be. He has not the power of attraction required to produce the perturbations in the movements of Uranus which gave rise to the search for an outer planet. M. Leverrier made his calculations under the belief that a planet of $\frac{1}{9300}$th part of the mass of the sun was required to produce the perturbations that had been observed in the orbital motion of Uranus ; whereas the planet discovered has only $\frac{1}{20.000}$th of that mass—not one-half of what was required. On the other hand, the semi-axis major of the orbit of the planet discovered is found to be 30·037 instead of 36·154 (Bode's law measures) used for the search ; which greater proximity to the sun, it is true, increases its power of attraction 1·449 times, but as its mass is only 0·465 per cent. of what was expected, the attractive force would amount to less than 0·68 per cent. of what was required. Then the question comes to be, Where did the wanting 0·32 per cent. of attractive force come from ? And the answer is that some astronomers have been searching for another planet to make up the weight, with more or less diligence, ever since the deficiency came to be recognised. But all that we want to have to do with the question is to suggest a very plausible reason for the anomalous position of the orbit of Neptune.

If there is another planet beyond Neptune, the ring (perhaps the rings) out of which he and the others were made, must have been much greater in breadth than what we have assigned to it at page 88, viz. 1,010,000,000 miles ; perhaps even one-half more, as may be deduced from the addition made to Table IX., and what we have said in connection with the semi-axis major adopted for the sought-for planet, by M. Leverrier in his calculations. Now, that a ring of such enormous breadth should have held together in one piece, until it finally broke up through condensation and contraction, requires an extraordinary effort of imagination, after seeing what has taken place with the rings of Saturn; even the breadth of 954,000,000 miles appropriated to the Uranian ring (see page 90) demands an elastic imagination to conceive its holding together; so that the outer ring of the system may very well have been divided into two, as we have said at page 134, and two not very unequal planets made out of it—one into Neptune, and the other into one as far beyond M. Leverrier's adopted distance of 36·154, and of such mass as would make up the missing 0·32 per cent. of deficient attractive power. No doubt the outer ring may have broken up into several planets, or even into a swarm of asteroids, but we prefer to think of only two planets; because it seems to us that to draw Uranus into the position he occupied when Neptune was discovered, the two planets must have been operating in conjunction; an idea that is not so easily entertained when there are several planets, or a host of asteroids, to be taken into account.

We have already discussed, at page 115, the mode of formation of the sheet of matter abandoned by the nebula, its posterior division into separate rings, and how the part of these rings from Saturn inwards could revolve themselves into planets having direct motion, so it is not necessary to go over the same ground again, merely because we are dealing with a hollow nebula instead of one full of cosmic matter to the centre.

We have also shown, at page 119, that the nebula must have been somewhat in the form of a cylinder terminated at each end by what may be looked upon as a segment of a sphere, although it would more probably be an almost shapeless mass of cosmic matter, because the greater part of it would be very slowly brought under the influence of centrifugal force as it fell in from the polar directions; and again, a few pages back, that almost all the matter coming in from its equatorial regions—even what might be called its tropical regions—would be intercepted before it could reach the Saturnian nebula. Likewise, at page 137, when examining Bode's law reversed, we have seen a limit set to the acceleration of the movement of revolution in the planets of the system as they approached the centre, because any acceleration beyond a certain limit, clearly marked out, would of necessity be within the nebula itself, and the rate of revolution would be less than that of the sun on its axis at the present day. This may be used as an argument against the nebular hypothesis, but we think we have shown in the same

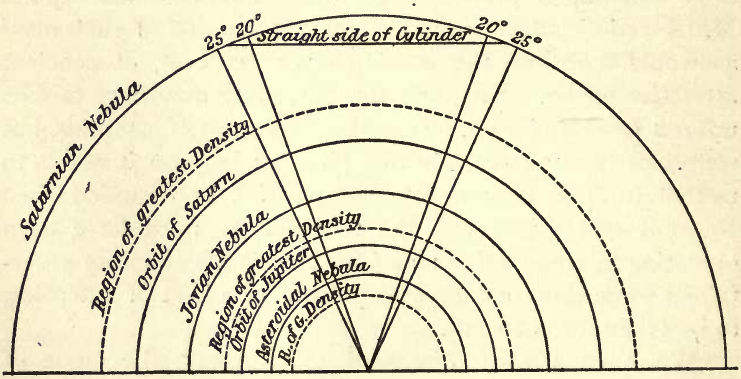

FIG. 2.

Chapter VII. that this is not the case. But we have still to try to account for the repeated rises and falls in density in the planets from Neptune to Mercury, or even farther ; which operation causes us to bring forward, first of all, a new idea as to what the form of the nebula would come to be.

The accompanying rough sketch (Fig. 2), drawn to a scale of one-quarter inch to 1,000,000,000 miles shows that, supposing the Saturnian nebula to have been a perfect sphere, and to have abandoned matter till the velocity of rotation came to be equal in a region corresponding to the tropical region of the earth, the cylindrical part of it would present a straight side of more than 1,000,000,000 miles in length ; provided always that the general diameter of the nebula did not decrease through condensation and contraction during the operation ; but as this could not be the case the length of the cylindrical part would be considerably less than that. How much less we have no means of calculating. On the other hand we have seen, when discussing, in the case of Jupiter, how matter must have been abandoned by any nebula, that from the time the original nebula began to abandon matter through centrifugal force, it must have gone on acquiring a constantly increasing length of straight side as it contracted. Thus the Saturnian nebula would begin work with the accumulated cylindrical length it had inherited from Neptune and Uranus, so that the straight side may have been very much longer than that shown by the sketch ; a simple look at it is enough to make one believe that this would be the case. But this idea naturally leads us to another digression.

Looking again at Fig. 2, we see that acceleration of rotation in the nebula would originate where condensation was greatest, that is at the region of greatest density, and have to be propagated from there to its periphery so that it would reach the middle of the cylindrical part sooner than the ends ; and as the nebulous matter at the ends of the cylindrical part could not be abandoned until it had acquired the centrifugal force necessary to overcome gravitation, it would lag behind and overhang, as it were, the middle of the cylindrical part ; which means that instead of continuing to be straight, the line of separation between the nebula and the abandoned matter would come to be concave ; and in this manner the nebula would soon assume the form of a dumb-bell, gradually becoming more and more pronounced as condensation proceeded. One can hardly help concluding that this must have been the way in which the dumb-bell nebula near star 14 Vulpeculæ was formed. The representations of it given by Chambers, Vol. III., page 92, Figs. 76 and 77, as seen by Smyth and Sir John Herschel are most confirming of this idea ; notwithstanding the changes of appearance shown by Lord Rosse's reflectors of 3 feet and 6 feet diameter, Figs. 78 and 79, which are not difficult to account for. It is easy to imagine how Fig. 78 could be converted into Fig. 79 when observed with a much more powerful telescope. We can conceive the roundest end of it being reduced into the sort of compact segmental form on the left hand side of the figure, and the spread-out part of it into the more diffused segment on the other side; but the form of the whole figure forces us into another conception. Mr. Chambers says the general outline resembles a chemical retort, but to our eyes it is infinitely more like one half of a dumb-bell broken off from the other. So like it that we feel inclined to ask what has become of the other half. This again makes us think of an enormous dumb-bell nebula dividing itself into two parts, one of which has disappeared or broken up in some manner without leaving any distinguishable traces of its existence, and the other, either forming itself into a double star, assuming in the process the form of a dumb-bell, or actually of one rotating in a direction almost at right angles to that of the original one; more probably the former of the two. Perhaps we have allowed our ideas, or fancy, to run on too far; nevertheless, looking over the forms of nebulæ represented in Chambers's classical work, and duly considering how inconceivably strange some of them are, there is nothing impossible in all we have said.

Returning to the repeated changes of density in the solar planets, we know that the matter first abandoned by the original nebula, through centrifugal force, would be at the lowest stage of density, and that what followed would go on gradually increasing in density as it contracted to the Saturnian nebula. But, as we have shown that immense quantities of matter belonging, so to speak, to the sun, though actually separated from the original nebula, must have fallen in upon the sheet after being abandoned, it is not difficult to see that the part of the sheet out of which Neptune and Uranus were made, might be more dense than the Saturnian nebula, on account of this matter being added to it; and that, as the greater portion of it must, at the more advanced stage of the process of condensation, have fallen upon the Uranian part of the ring, because the space from which it fell would be higher, the density of that would be greater than the Neptunian part of the sheet; both of them exceeding the density of the Saturnian nebula. Again, we have supposed, very naturally we think, that all extraneous matter coming in from the equatorial direction would be intercepted by the rings destined for Neptune and Uranus, so that the density of the ring for Saturn would be only what had been acquired through condensation, and the planet formed out of it would be less dense than those made out of matter accumulated in a different way. It may be argued against this deduction, that density would depend on the degree of contraction, but it is natural to think that lighter would take longer time than heavier matter to condense to the same degree; besides Saturn is of necessity the youngest of the three planets, and may in due time come to be as dense as either of the other two, but his diameter will decrease proportionately.

Coming now to the Jovian nebula, whose diameter we have made to be 1,370,000,000 miles, we have seen, at page 115, that—had it been a perfect sphere—by the time it had contracted one thousand miles in diameter, it must have had a flat side of more than 1,400,000 miles in length? then if we add to that length all that the nebula had inherited from Neptune, Uranus, and Saturn, the cylindrical part of it must have been many millions of miles in length, and the polar very much greater than the equatorial diameter of the nebula. In other words we have to deal with a body having the form of a very long cylinder terminating in spherical caps. To this we have to add that the density of the Jovian was more than 111 times greater than that of the original nebula. Still farther we have to take into account that the whole of the matter abandoned by that nebula must have been thrown off in less than one-half of the space in which the ring for even Saturn had been abandoned, the breadth of the two rings, as shown by us, see Table III., having been 650,600,000, and 313,400,000 miles respectively. All these things considered, it is clear that the thickness of the ring for Jupiter's system must have been very much greater than what we have given it in the table; which, coupled with its matter being over six times more dense than that of the preceding ring, is sufficient to account for the rise in density, the immense size, and mass of Jupiter.

Next, we have the means of accounting for the fact that, the space occupied by the Asteroids is, and has always been, the least dense of any portion of space occupied by the solar system. It is easy to understand that the enormous mass of matter abandoned by the nebula for the formation of the Jovian ring—more especially towards the end of the process—would have a very appreciable effect, by its attractive power, in helping centrifugal force in freeing matter from the power of gravitation; the consequence of which would be, that the matter thrown off for the formation of the Asteroidal ring would be considerably less dense than it would otherwise have been. In this way, then, we have the decrease of density, as well as the quantity of matter, in that space very plausibly accounted for.

Then, as the nebula continued to contract, the attractive power of Jupiter's ring would decrease proportionally to the square of the distance of the receding mass, ceasing in doing so to lend so great assistance to centrifugal force in the nebula, and so letting it subside into its normal state; so that the matter abandoned would increase in density in comparison to the space over which it was distributed, thus accounting for the rise in density towards Mars and the Earth.

With regard to the fall towards Venus and final rise towards Mercury, we have to take into consideration the anomalies—already taken notice of—in the dimensions, densities, etc. etc., of the two planets Earth and Venus; it being, we may confidently say, certain that the whole of them have arisen from the same causes. Following up the idea of a dumb-bell nebula—as we might have done in the case of Jupiter also—as the breadth of space for receiving matter abandoned by the nebula went on rapidly decreasing, the thickness of the ring left behind would go on increasing, and the overhanging matter of the dumb-bell would be deposited always in greater quantity on the outer than the inner part of the ring as it broadened; we can conceive that the whole extent of the sheet of matter allotted to the Earth and Venus would be thicker at the outer than the inner part. Hence, when this part of the sheet came to be divided into two parts for the formation of two planets, the outer would naturally be the greater and denser of the two, and thus occasion the rise in density from Mars to the Earth, and the fall to Venus. Finally the rise in density to Mercury would be only the beginning of the gradual, and final, rise to the sun as it is at present.

If the idea of a nebula in the form of a cylinder with hemispherical ends is admitted as possible, or somewhat like a dumb-bell, the extreme diameters of the 9 successive nebulæ we have dealt with would be considerably different in their equatorial directions to what we have given them, although their polar diameters might continue to be not far from the same; but that would have very little effect on the operations we have gone through, seeing we have shown that there could be no actual divisions between them such as we have adopted; and that the division of the sheet of matter abandoned into separate rings must have been brought about by some means which we cannot explain; a process, nevertheless, which has been subject to some law, or laws, operating evidently in a regular and steady manner throughout the whole time, during which the matter was being abandoned, as is proved by the general uniformity, or harmony, in the distances of the planets from the sun. Should anyone come to be able to account for the division of this sheet of matter into distinct and separate rings, he will also be able to account for the acceleration of rate of revolution from one planet to another, and for the anomalous rates in the cases of the Earth and Venus.

In a former part of our work we have followed up, at different stages, the condensation of the original nebula until it attained the dimensions, appearance, and some of the features of the sun as it is, but we have still something to add as to how the condensation could produce a body so strictly spherical as the sun is represented to be. All the other bodies of the solar system, as far as astronomers have been able to measure them, are spheroids more or less oblate, and it seems strange that the principal should be the only one that does not conform to the general figure. It is rather hard on the notion that the original nebula gradually assumed the form of a lens, for it would require a special mode of manipulation of a very mechanical kind, rather than the steady, imperceptible self-action of the law of attraction, to transform a lens into even an oblate spheroid; to transform it into a perfect sphere would be absolutely impossible. For, if at the end of the process it was found that there was too much material to form a sphere, it would be hard to get rid of the superabundance, unless it was converted into meteorites—evidently another hand process. On the other hand, should a hole remain to be filled up, the material would have to be lugged in somehow from some of the errant masses, or lambeaux, which impact-theorists find it so easy to have at hand when required. Let us then think of why and how it came to pass that the sun is an almost perfect sphere.

If we suppose that, when cosmic matter ceased to be thrown off by it, the form of the nebula was that of a cylinder terminating in semi-spherical caps at the ends, it requires no great stretch of imagination to conceive that, between attraction and centrifugal force, the whole mass should be converted through time, first into a prolate spheroid, and then into a perfect sphere. And very possibly time only is required for the sun to become an oblate spheroid, the same as his dependent planets.

Should this form of nebula not be admissible—and we can see no mechanical reason why it should not—and we are thrown back on a lens-shaped nebula, the only resource left us is to suppose that through continued action of attraction, and of centrifugal force, or rather revolution constantly increasing, the latter gaining the victory over attraction, finally converted the lens into an actual ring, something of the nature of the ring in Lyra; and that that ring, no longer increasing in revolution, would have to yield to the law of attraction, and would condense and contract and close up into an oblate spheroid, and then into a sphere. It is a roundabout, rather fanciful, process, but any other way of converting a lens-shaped nebula into a sphere, under the law of attraction, is absolutely impossible.

CHAPTER XVII.

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AT the end of Chapter VII., when making some remarks on the heat of the sun produced by gravitation, we said that according to the areolar law the condensation produced thereby would originate difference of rates of rotation in the nebula—provided it did rotate as Laplace assumed—depending on its degree of contraction and consequent density increasing as the centre was approached; and that these differences of velocity of rotation would give rise to a churning action in its interior which, owing to the friction caused thereby amongst the particles of its matter, would produce heat over and above what was produced by gravitation alone. Again, at the end of Chapter XII., we said it would not be difficult to show what tremendous commotions throughout the whole nebula would be produced by these differences of rotation; but that this could not be properly done until we had reconstructed the original nebula, and had shown how from it the solar system might be constructed. Now, therefore, that we have set forth, as fully as we can, our ideas of the formation of a hollow nebula and the construction from it of the solar system, we shall proceed to show how heat was, and must still be, produced by the churning action, over and above the definite quantity that could possibly be produced by simple gravitation. And also to show how our notions of the interior of the nebula first, and afterwards of the sun, are simplified and made more natural by looking upon it as a hollow sphere.

We will begin by considering, first, what would take place during the contraction and condensation of a rotating nebula solid to the centre—i.e. filled with cosmic matter to the centre—as that is the condition under which such a body has been studied hitherto—as far as we know at least..

Not to weary humanity—our own included—by repeating, what almost every one knows, who the parties were and how they came to the conclusion, that by far the greatest part—almost the whole—of the heat expended by the sun, ever since it had any to expend, has been produced by condensation caused by gravitation; we shall for the time being accept this as the general, almost universal, opinion at the present day. If any proof of this being the case is considered necessary, we have only to appeal to Sir William Thomson's lecture, delivered at the Royal Institution on January 21, 1877, in which he showed how a cone of matter, similar to that of which the sun is made, with base at the surface and apex near the centre, falling into a similar hollow cone excavated in his body, would, in descending a certain distance, generate as much heat as would maintain a proportional part of his expenditure for a year; and in which, beyond stating that a very small part might be produced by the fall of meteoric matter on his surface, he makes no mention whatever of any heat-producing power except gravitation pure and simple. The weight of the cone falling into the conical pit alone, produced almost the whole of the desired supply. That this manner of calculation is one of those modes which, as we have said from the very beginning of our work, could never have been adopted had a little more thought been expended on them, can be easily demonstrated even in the case we are now considering. This we say with all due deference to so great an authority; more especially as we know how difficult it is, how seeming unnecessarily laborious, to examine everything to the very bottom; and how pleasant and satisfying it is to feel contented, when we have obtained what suits our purpose.

When we began to consider, in Chapter XV., what would be the interior construction of the nebula, we supposed, at page 269, that it had assumed a somewhat globular form when its diameter came to be three times that of the orbit of Neptune, which would be 16,764,000,000 miles; and we will return to that supposition to set forth our conception of how heat would be produced in a nebula of that diameter solid to the centre—that is full to the centre of cosmic matter. In that case a particle of matter starting from the surface, under the power of gravitation, would have to travel 8,382,000,000 miles before it reached the centre, and would carry with it a constantly increasing power of producing heat, derived solely from the action of gravitation. Next, we have to consider what would stop it when it reached the centre and enable it to give out its heat—for until it was stopped it could give out no heat at all—and the most easily conceived means of stoppage would be to suppose that an equal and similar particle coming in from exactly the opposite side of the nebula met it there. If it was not that it would be something equivalent and much more difficult to describe, while the result would be the same. The result would be that, as each particle came in with equal power of producing heat, the

the amount produced when the two met and stopped each other would be just double what each of them brought with it; that is our way of looking at it at least, considering that the velocity with which they met would be just double what each brought with it, and the force of the shock would be double what it would have been had only one of them been stopped in some other way; that other way would have had to give or furnish its half of the shock, and would therefore be able to give out as much heat as the stopped particle. Whether two of Sir William Thomson's cones meeting at the bottom of his pit, from exactly opposite sides of the sun, would have the same effect as we have found for the two particles, may perhaps give rise to the discussion; but we do not see why the result should be in any way different. When a stone falls from a height upon the earth it gives out, in the form of heat, all the heat-producing power it had accumulated in its fall, but we are apt to forget, perhaps have never thought at all of, the why and the how it gives it out, especially of the latter. The why is because it is stopped, and the how is by the earth coming to meet it, and these two ways have an inseparable relation to each other. And if the earth comes to meet it, which it most undoubtedly does, though we cannot measure how far it travels, it must bring along with it an amount of heat-producing power equal to that possessed by the stone, when it in its turn is stopped by the stone; thus the amount of heat arising from the fall of a stone to the earth is, apparently, just double what it is usually estimated to be. This fact comes under the category of splitting hairs or, more truly speaking, of negligible quantities; but the whole mass of the sun falling to the centre cannot enter into that category, and whether we will or no we have to take it all into account.

We have conducted two particles of matter from exactly opposite points of the surface of the nebula to its centre, and shown that by simple gravitation a certain amount of heat would be produced by them when they met there and stopped each other; now, we propose to conduct two particles, not far from each other, from one side only of the nebula to the centre, and point out what would happen to them on their voyage thither. The road is long, as we have seen, and during their voyage there would be time enough for a good many things to happen, but we shall only take notice of two for the present, namely, gravitation—of which we have already almost disposed—and attraction; for as far as their journey is concerned there is a very marked difference in the meaning of the two words. Gravitation—that is, the action of a ponderable body falling—acts only in a straight line from any point to a centre of attraction, while attraction acts in every possible or imaginable direction. We have already seen what happened to the first particle despatched to the centre under the power of gravitation alone, and have only to say, that the same would happen, under that power, to the two we have now in hand; but attraction—actually the father or mother of gravitation—would have a good deal to do with their journey. From the moment they started—very likely they were practising before they left—they would rush at and continue to bombard each other during the whole voyage. At each encounter or collision, however caused, a certain amount of heat would be produced in each of them which they would carry along with them, and would augment the gravitational quantity they would have to give out when stopped in their fall, in the way we have pointed out would be the only one that could bring them to rest. It may be said that that heat would be left behind in space on the way but space cannot absorb heat unless it contains something to hold it in, and that something could only be similar particles of matter on the same voyage, also creating heat and having as much to dispose of, no doubt, as the two we are conducting. This lateral attraction, so to speak, is really what instituted rotary motion in the nebula, and produced the differences of rotation and the churning action in it with which we shall have to deal presently.

Having passed under examination the quantity of heat produced by the contraction and condensation of the solar nebula into a globe solid to the centre, we have now to do the same for the case of its being a hollow sphere, and we may say that our work has already almost come to an end; for we have only to vary to a small extent what we have just set forth. Beginning then as before, with one particle of matter falling, or rather being attracted, from the surface of a hollow-sphere nebula, we find that it would not reach the centre at all, but would be stopped by another drawn out from the centre by its own attraction, which would meet it—say for brevity—half-way between the starting points of the two, each bringing along with it its own heat-producing power and giving it out to its opponent, there being nothing else to give it to; so that if each brought with it x heat-power they would have $2x$ heat-power between them, just as we have said would happen in the first case, and the heat of each one of them would consequently be doubled. In this case we have to observe, though it is really unnecessary, that as yet we have spoken of attraction as acting in one direction only, that is, in doing only the work of gravitation; so we have still to consider the voyage of two particles of matter proceeding from the surface and meeting two coming from the centre, and have only to say that their mutual collisions caused by lateral attraction on the way, would enable them to bring along with them certain quantities of heat produced by these collisions, which would be over and above what they acquired in their straight-line imaginary voyage.

If any one doubts that additional heat would be produced by this lateral attraction and bombarding, let him take two hammers and strike the one against the other as rapidly as he can for some time, and he will be able, by touch, to convince himself that heat can be produced by this lateral attraction as well as by the *attraction of gravitation*; and, if he could measure it afterwards, he would find that if he dropped the hammers on the ground, they would not give out any of that heat but only what they had derived from gravitation in falling from his hands to the ground, unless the ground was colder than they, and if the ground was not colder, the heat it had would be augmented from this source also.

If the heat produced in both of the cases we have been examining caused differences of rotation in the nebula—as we have said on a former occasion—increasing in velocity as the region was approached where the stopping process came into action, it is clear that these differences would be greater near that region in a hollow-sphere nebula than near the centre of a solid sphere; for the reason that the particles of matter would there have more freedom, that is, more room to act in. We have shown that in the solid sphere the particles would come to be more or less inert, in proportion as they approached the centre; and also that in a hollow-sphere nebula no particle could ever come to be near to a state of rest, but that each could be freely driven by the collisions produced by lateral, angular, universal attraction over every part of the hollow shell—an effect that could by no means be produced in a nebula solid to the centre. We, therefore, think that there would be more life-power in a hollow-sphere sun than in the kind of sun from which all calculations of length of life have hitherto been made—at least, as far as we know.

It will be understood that we have spoken of particles of matter being stopped, or stopping each other, before they could give out their heat, only for facility of explanation; for no particle of matter can ever be brought to absolute rest, until all its heat and heat-producing power, *i.e.* motion, could be taken out of it, and that can only be when it is reduced to absolute zero of temperature. Cosmic matter could be reduced to the state of rock or steel, but its particles would not be at rest then, or else our ideas of the nature and construction of rock and steel are very erroneous; but it must be acknowledged that it would be much more easily reduced to the state of rock in a body solid to the centre than in the shell of a hollow sphere. In fact it is difficult to conceive how matter could exist at the centre of the sun at the present day without being as solid as rock, considering the enormous pressure it must be subjected to there, if its whole mass is condensing to the centre. But although the particles of the nebula could not be absolutely stopped, they might be so far retarded in their velocities derived from attraction that they would give out heat to each other, and wherever a collision took place there heat would be made evident, and condensation might take place. Particles of matter would not have to fall to a centre, but only to a a meeting place, in order to condense and create heat, and might form layers of condensation anywhere between the centre and the surface, either in a solid or hollow sphere, which would ultimately, even in the former case, form a hollow shell, as we have supposed, at page 274, might be the case. For even a small sphere formed around the centre in that way would be hollow, and would be undone when the different concentric layers approached each other, under proportionate forces of attraction, and formed into one hollow sphere. Thus we again come to the conclusion that the formation out of cosmic matter acted upon by the law of attraction, of a sphere full of that matter to the centre would be a mechanical impossibility. In either case the total quantity of heat produced by the contraction and condensation of the nebula would include, not only what has hitherto been looked upon as belonging to gravitation alone, but that other part derived from attraction in all other directions. So the age and duration of the sun still remain to be estimated.

We have not said, but we have not forgotten that it may be said that, if in Lord Kelvin's estimate of the sun's heat, a cone of matter falling in from one side of it was stopped by a similar cone falling in from the exactly opposite side, one half of the sun's mass stopping the other could only produce the amount of heat calculated by him. Neither do we deny that the same may be said of the two half-volumes of the sun meeting at the region of greatest density in a hollow sphere, and that the amount of heat produced by gravitation alone would be the same in both cases. All that we have wanted to show is that, in addition to the quantity so produced, the quantity produced by lateral attraction, so to speak, has to be taken into account, in order to estimate the total quantity ever possessed by the sun.

Referring now to what we have said towards the end of Chapter XV., of rotary motion being instituted at the region of greatest density of the nebula, and being propagated from there to all parts both outwards and inwards, we can at once account for the different periods of rotation observed on different parts of the surface of the sun; and not only that, we can assert that these differences of rotation must exist throughout the whole volume and mass of its body up to the present day. We have no need to appeal for producing them to showers of meteors falling on its equatorial regions; neither do we pretend to say that such showers have no part in producing them; but we do say that the part they play in the affair, and the depth to which they can penetrate into the sun's body, must be altogether insignificant compared to what we have pointed out as the true and indisputable cause.

We may now proceed to consider what would result from the commotions produced by these differences of rotation in the interior of the sun, and we shall begin by observing that an enormous amount of heat would be produced thereby. The churning action, as we have called it, must be of a very formidable character, for, supposing the whole of the interior to be in a gaseous or gasiform state, it must be effected under a pressure of not less than 28 atmospheres at the surface, and at what pressures as the centre is approached no one can tell; and if the matter in the interior is in a viscous condition, the friction caused by the churning will only be the greater. But let us try to form an idea of what the force, or rather violence, of that churning action must be in the sun if constructed in the manner we are advocating; for which purpose we have to form some definite notion of what is the difference of velocity of rotation at different parts of its circumference, which can hardly be better shown than by Table X., in as far as these rotations have been approximately measured.

The first thing to be observed in the table is that the rate of rotation at the equator is 75·10 miles per minute, and that at Lat. 45° it is only 48·23 miles, giving a difference of 26·87 miles per minute in one-fourth part of the sun's circumference, which is a velocity 27 times greater than our fastest express trains. And the next is to note, in the last column, how these 26·87 miles of difference, when divided over spaces of 5° each, show decreases in velocity of from 0·39 at Lat. 5° to 5·06 miles between degrees 40 and 45.

A little thought bestowed on these two points will show what commotions must be produced at the surface by this enormous variation of rotation and make us speculate on how much greater it must be near the poles than at the half distance from the equator. Then, if we look upon the sun as a hollow sphere we have to consider that, according to the theory that the condensation of a nebula increases its rotation in proportion to its approach to the region of greatest density, of the velocities of all the rotations expressed in the table, the greatest must be at that region, the others diminishing from there outwards to those of the surface, and inwards to almost nothing at the centre; for we have seen that there must be gases enclosed in the hollow, and that motion must be communicated to them, through friction, down to the very centre. Taking all these things into consideration, it is certain that the churning must be very much greater than anything we have thought of up to the present moment, the commotions created more tumultuous, and the heat produced by friction incalculable.

TABLE X.—Showing the Differences in Velocity of Rotation of the Surface of the Sun, at Distances of 5° from each other, from the Equator to 45° of Latitude.

Latitudes. Degrees.	Circumference at each 5 degrees of Latitude from 0° to 45°.	Time of Rotation in Days.	Rotation per Day in Miles.	Rotation per Hour in Miles.	Rotation per Minute in Miles.	Retardation in Miles per Minute for each 5 degrees.
0	2,723,767	25·187	108,142	4506	75·10	..
5	2,713,367	25·222	107,581	4483	74·71	0·39
10	2,682,387	25·327	105,910	4413	73·55	1·16
15	2,631,058	25·500	103,170	4299	71·65	1·90
20	2,559,504	25·737	99,441	4143	69·06	2·59
25	2,468,572	26·040	94,799	3950	65·83	3·23
30	2,358,852	26·398	89,357	3723	62·05	3·78
35	2,231,179	26·804	83,242	3468	57·81	4·24
40	2,086,526	27·252	76,564	3190	53·17	4·64
45	1,925,994	27·730	69,455	2894	48·23	5·06

Note.—The times of rotation are taken from Messrs. Newcomb and Holden's "Astronomy," p. 290.

Lest we should have been misunderstood in what we have said a few pages back, and it be thought we consider that all the heat produced by this churning action ought to be added to that produced by gravitation alone, when attempts have been made to compute the total quantity ever possibly possessed by the sun, we have to insist that the idea of gravitation in itself—that is, of matter falling to a centre—is altogether erroneous in connection with the construction of the sun from a nebula, and that it is in truth utterly misleading. We know perfectly well that in the construction of the sun, heat could only be produced, in the main, by bodies colliding with, or rubbing against, each other, and that a large part of that produced by universal attraction must have been expended in producing rotary motion; but we also know that in its construction no particle of matter can ever, as yet, have been brought to the state of rest of solid matter even, that it has still the power of colliding with its neighbours and of producing heat, and that it will continue to preserve that power until it is bound up into a solid state along with its neighbours. Even then it will not be absolutely at rest, but will have lost its heat-producing power, and will begin to lose the quantity it then possesses when it gets permission from its neighbours. It is a fallacy, therefore, to suppose that the matter of which the sun is composed has no other heat-producing power than what is derived from its fall, through gravitation alone, from the potential position it held to the centre of the incipient nebula. The only end to heat-producing power is fixed position.

If science chooses to fix that position at the centre of the sun, or as near to it as successive particles can reach, there must be any quantity of it in a solid state even now in that neighbourhood, if due consideration is given to the pressure it must be subjected to there. If it chooses to entertain the idea of the sun's being a hollow sphere, somewhat in the form we have described, there can be nothing in its whole body so dense as even water up to the present time. In the first case it has to remember what we have done our best to prove: That *gravitation* ceases to act when a body falls to a fixed centre or position and can fall no farther. From there it cannot rise except through upper or exterior attraction, and in that case it would leave a hollow space in the place it had occupied. It is altogether illusory to dream of convection currents where no means or force of any other kind than attraction could give rise to them, in which case we should have attraction and gravitation working against each other, two things that have been confounded into one turning out to be antagonistic, as no doubt they sometimes actually are—as we have shown when treating of the discovery of Neptune—but when they are so, they never can produce convection currents. In the second case in which, as we have seen, there can be no matter at all near to the solid state or fixed position up to the present day, we can conclude that the life of the sun, measured by heat-producing power, must be very much longer than in the first case, in which a very large part of the matter of which it is composed must have lost that power ages ago.

We have still to bring to mind what we have said in Chapter XV. of the region of greatest density of the nebula being the region of greatest activity and greatest heat; and to add now, that the whole space between that region and the centre must have been acting as a reservoir—partly material, partly gasiform—of heat, ever since the nebula began to contract and condense, quite independently of its carrying before it the minus or plus sign. From that time that region would be the regulator of the radiation of heat into space, or to wherever it was radiated; because no heat produced on the inner side could escape into space without passing through and acquiring the temperature of that region, or first giving out to the outer side any greater heat that it might have produced and accumulated; facts which involve the necessity of the whole of the interior space, or volume being heated up or lowered down to the same degree before any of it could be transmitted outwards. Thus, in addition to all we have said of the means of lengthening the sun's life, we have to take into consideration that all heat radiated from the surface must be conducted, or carried somehow, through a distance somewhere between about 2,000,000 and 90,000 miles, before it could escape into space or elsewhere, according to when it began to be radiated at all. And we have also to take into consideration the probability that the heat produced and accumulated in the inner half of the volume would, by its repulsive force, retard the condensation of the nebula, and thus prolong its heat-giving life.

Looking back on our description of the construction of the sun, how rotary motion was established in it, and how that motion has produced the different velocities of rotation, not only on the surface where they have been observed and measured, but which must penetrate to the very centre ; we may now proceed at the expense of some repetition—in which we have already somewhat indulged—to show how our mode of construction and development enables us to understand a great many things that have been observed in it, much better than we have been able to do from any explanations that have hitherto been available. It gives the most satisfactory reason possible for the sun-spots occupying principally two zones at marked distances from the equator. There is one belt round the equator of 16° to 20° wide on which we know, from Table X., that the differences of velocity of its edges and of those of the contiguous zones, one on either side, hardly exceed 1 mile per minute. Towards the poles there are two segments measuring from 80° to 90° broad, at the borders of which the rotary velocity is slower by 26·37 miles per minute than it is at the equator, and 5·06 miles per minute slower than at 5° less latitude, as also shown by the table. And between the central belt and these segments there are two belts or zones, each 30° to 35° wide, in which sun-spots are almost only to be found. In these two zones the churning of the interior would be in all its vigour, most probably more active at their centres than where they meet the central belt and the polar segments ; where our knowledge of the diminished velocity ceases, but where we have no reason to suppose that it actually stops.

Were the period of rotation the same throughout the whole body of the sun—with the exception of what has hitherto been considered to be a mere surface difference produced by external causes—we could conceive that the heat produced solely by condensation would find its way to the surface equally in all directions, even bubble up all round like steam rising from the surface of the water in a boiler, in this way forming what is called the sierra; and that there would be neither sun-spots nor eruptive prominences, hardly any of the violent movements recorded in works on astronomy. But the churning action we have been exhibiting, extending to the deepest recesses of the sun, must produce commotions quite adequate to give birth to the most violent phenomena that have been recorded. Viscous gases and vapours, gasiform vapours, ground against each other at depths of hundreds of thousands of miles, under pressures of hundreds, much more likely of many thousands, of atmospheres, and confined by superincumbent strata, so to speak, would acquire a dynamitical explosive force that could be conceived to be powerful enough to rend the sun into fragments, were it composed of anything comparable to solid matter. On the other hand, the friction of the solar matter operated under the pressure of 28 atmospheres at the surface, and up to the unknowable number at the greatest depth, converted into heat, would have explosive energy enough to give rise to all the phenomena that have been observed; from the veiled spot to Professor Young's prominence, which was thrown up to the height of 350,000 miles above the photosphere.

A veiled spot seems to be one that has broken through the photosphere, perhaps not even entirely, but not through the light or white clouds which float immediately over it; which, in consequence, goes a long way to prove that sun-spots have their origin in up-rushes of heated vapours from beneath; for a downfall of cooled metallic or other vapours would break through the light clouds first of all; and which is confirmed, as far as anything in solar physics can be confirmed, by what we are exposing. That there is a down-rush also, goes without saying, because there is no other way of giving account of what becomes of the vapours of metals and other elementary substances brought up by the outpours of heat, after they are cooled in the solar atmosphere. That they should fall down into the same opening they had made in rising up, is the most natural supposition that can be made; for, otherwise, they would have to be carried beyond, or outside of, the spot before falling. Moreover, a sun-spot is said to be generally surrounded by prominences which bring up vapours of elementary substances, that we must believe to be much heavier than those from eruptions of sun-spots, because they issue much more violently, showing that they must have been expelled by much greater force, which must form a sort of wall all round the spot through which the matter, thrown out by it, would have to be carried before it could be deposited; and outside these walls there are no visible signs of where it falls, so that we are forced to believe that all the substances, those from prominences as well as those from sun-spots, fall into the same general receptacle. Surely it could not be argued that there can be no eruptions from a sun-spot, seeing that the force required to drive matter through it must be less than when it is expelled from depths very much greater than the depths of the spots. Thus we have both up-rush and down-rush in sun-spots accounted for very plainly; and they are always large enough for both operations being carried on at the same time. Besides, they have been credited by eminent astronomers with the faculty of sucking in the cooled vapours from the surrounding prominences into the common pit.

In some sun-spots, said to be about 3 per cent. of those observed, cyclonic motions have been observed in the umbræ and penumbræ, which under the churning process might be expected to be universal in all of them, but it is not necessarily so; even leaving out the consideration of the difficulty of detecting them. We see in a deep smooth-flowing river eddies revolving in all directions, caused by currents of different velocities approaching each other, quite independent of the form of the banks of the river or obstructions in the places where we see them, but without doubt derived from sources of that kind higher up in the river; and so it may be with cyclonic motions in the sun-spots. The velocity and direction might be given to the vaporous matter by the churning action before issuing into the spot, which would cause eddies in it in all directions, the same as those in the water of the river. It would be absurd to think that in a space so immense as the bottom of a sun-spot, there should be only one orifice of emission of vaporous matter: there might be any number; consequently, there may be times when the out-flowing currents annul each other and none at all are seen, or when there are partial currents in any direction; others when they may be all so uniform as to produce a cyclonic motion all round a spot, or nearly all round it, or two or more in opposite directions, all as has been recorded on more than one occasion. Neither could it be supposed that any cyclonic motion, caused by the churning, could depend on which side of the equator the spot was formed in. There must be little churning going on under the surface at the equatorial belt, hence the paucity of spots there; but between the surface and the centre there must be some point of meeting of the motions that are produced on each side of the equator which, even were there no special reason for it, would destroy all chance of uniformity, or distinctive direction, in the upheaved matter when it arrived at the surface, let it reach that place on whichever side of the equator it might. The original salient motion at the bottom of a sun-spot might be to right or left, or according as the material from which it proceeded had been tumbled about, and the issuing motion might also be controlled greatly by the form and position of the orifice, or rather tunnel, through which it escaped. Common churning, we know, could not drive all the milk in one direction, even were the paddles of the churn solid; and in our case, the paddles have to be looked upon as even more divided, magnitude for magnitude, than they are in an ordinary churn, for the matter itself forms the paddles.

The cyclonic motions observed in prominences must come from the same causes, and ought to be more general in them, seeing that they must proceed from apertures much fewer in number than in the sun-spots, and very probably from one orifice in the case of jet prominences. One would expect also that these cyclonic motions would be more regular in the prominences, from being generated deeper down in the interior than those of the sun-spots, and less affected by the motions they encountered on their way out, owing to the great original energy required to force them through the superincumbent mass of matter, and might even have—in jet prominences especially—the motion to be expected according to the hemisphere from which they proceeded. But we have already said that, deep in the interior, the churning motion may be in any direction whatever. It is natural to suppose that the highest prominences are ejected from the greatest depths, because they require the greatest ejective force to throw them to such immense heights, and because the greatest ejective force must be where the heat and pressure are greatest, that is, at the densest and most active depths. And probably the reason why prominences generally surround sun-spots is that they have had their exits facilitated by the relief from pressure, brought about by the discharge of churned matter into them (the sun-spots), and thus, as it were, attracting the eruptions of the prominences towards them.

We had almost omitted to say that the churning theory would very well account for almost every sun-spot having more or less proper motion of its own independent of all others, and for all of them drifting towards the central belt, or towards the polar segments when they begin to dissolve and disappear.

There are many other things in connection with the sun that could be explained through our mode of construction, some of which are so evident that they will occur to anyone, and others that lead into depths too profound for us to enter.

To conclude. The construction of the sun we have set forth would be of great service towards the completion of either of what Professor A. C. Young calls the competing theories of M. Faye and Fr. Secchi, in which the former would find the origin of the solar storms, to which he appeals for producing sun-spots in particular zones, and a better way of accounting for the differences in velocity of rotation between the equator and the poles than in the depths of the strata between these regions ; and the latter the means of forming the dense clouds of eruption which he assumes to form sun-spots by settling down into the photosphere. But theorists seem to be partially right by a divination, and to have only failed through their not having found out the sources of the powers they called into existence, in order to have some foundation to build their theories upon.

CHAPTER XVIII.

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335 Most probably no matter in the sun exceeds half the density of water. The unknown line in the spectrum of the corona belongs to the ether.

WHEN we were attempting to describe in some measure the region of space from which the sun obtained the nebulous matter out of which it was formed, we found that it would produce a nebula somewhat resembling a most gigantic star-fish, with arms or legs stretching out from it in every direction, which might be likened to mountain-peaks rising from a tableland or range of mountains; and when we began to condense the nebula we concluded that these peaks would very soon, comparatively, be left behind the main condensation, owing to their being more under the influence of the attraction of surrounding suns. And we might then have added less under the attraction of the main body, on account of its gradually increasing distance arising from its greater rapidity of contraction. Now, we propose to return to these portions of the sun's property so long left out in the cold, to think of what in all probability became of them, seeing that they must all have had somehow a part of some kind to take in the formation of the solar system.

First of all, we have to form some idea, however vague, of their number, which may be divined to a very limited extent from the following considerations: We see, from Table VIII., that the sun's sphere of attraction extends to more than 4000 Neptune distances in the direction of *a* Centauri, the star nearest to the earth, which corresponds to 11 billions of miles. Then, although we have said, in Chapter XV., that instead of there being a peak on the nebula in that direction there would be a deep hollow in it, we shall proceed to find out what might be the diameter of the base of a peak at that distance supposing it to be somewhat in the form of a cone. We know that the moon does more than eclipse the sun, which is 867,000 miles in diameter; so, for facility of calculation, we may suppose that it eclipses a portion of space at its distance of 1,000,000 miles in diameter. Consequently, the base of a peak such as we are measuring would be eclipsed were it 129,000 millions of miles in diameter, and then only. Moreover, we have deduced the diameter of the base of such a peak from *one* diameter of the moon; so that wherever we see two stars only one breadth of the moon from each other, there we have room for at least one peak with a base of the above diameter. Last of all, when we come to think that there are as many as six to seven thousand stars visible to the naked eye, and of the intervening spaces between them, we have to conclude that the number of peaks surrounding the original nebula before they began to be left behind, or cut off, must have been almost beyond our conception; more especially if we look at Table VII., where we see that the star Canopus is 25 times farther from the sun than *a* Centauri. We are accustomed to look with wonder on the volcanic peaks of the moon, but they can do nothing more than give us an exceedingly faint representation of the original nebula seen from an appropriate distance outside, when it had begun to contract more rapidly than the peaks could follow it; seeing that we are comparing a diameter of 2,160 miles with one really almost infinitely greater.

Finding ourselves, then, with an innumerable host of peaks, or cones, of cosmic matter on our hands, we have to think of what can be done with them, and we begin by saying that the use to be made of them was suggested to us when we discovered the jagged nature of the domains of the sun. Some of them have been most probably swallowed up in the formation of the sun, and could we believe in the plenum of meteorites in all space, that has been fancied to exist by some physicists, we might derive its origin from a part of these peaks; but if there can be such a plenum in space, its origin might be much more naturally derived from a suggestion made in a former chapter, at page 258, to which we shall refer presently. In the meantime, looking upon the multitude of comets, meteor-swarms, etc., which revolve around the sun, or are supposed to exist somehow in its neighbourhood, it is very natural to entertain the belief that they have been made out of some of the most important peaks—or the refuse from them—that must have formed part of the original nebula. To deal with all of them when we cannot number them, or even with the six of Table VIII., about which we actually know something, is out of the question, so we shall only try to show what could be made out of one of them.

Confining ourselves, then, to the peak of *a* Geminorum, whose collecting ground had originally reached to 24,000 Neptune distances, or 67 billions of miles—this being the point of space where the attractions of the sun and that star balance each other—if we suppose it to have been contracted till its base was of the same diameter, and its distance the same from the sun, as that of the base of the peak we measured not many minutes ago, 129,000 million miles, and 11 billions of miles, respectively, we can easily conceive that its height may have been 10 times as great as the diameter of the base, or more than $1\frac{1}{4}$ billions of miles. Here then we have in the direction of only one star a mass of cosmic matter out of which something more than a comet, even of the grandest known to modern astronomy, could be made. Of its tenuity, all that we have any necessity to think is, that it would be much less—i.e. more dense—than that of the original nebula.

Beginning then with the dimensions we have just stated, we know that the attraction of the nebula would draw the matter of the base-end of the peak more rapidly towards itself than that of the apex-end ; we know also that there would be different rates of contraction going on in different parts of the length of the peak—for the same reason we have given for the peaks being cut off from the nebula ; so that the condensation throughout its whole height, or length, would be proceeding at different rates at different places, which would certainly divide the peak into several parts, perhaps into many. If now we suppose that the leading part of it—the one nearest to the nebula or sun—or even the whole of it, formed itself into a comet, it is not difficult to see that it might have a tail infinitely longer than any comet the length of whose tail has been measured.

There can be no doubt that in the whole length of the peak the action of attraction would be exactly the same as we have found it to be in the nebula itself; that is to say there is no reason why it should not come to be a hollow cone —comets are reported to be hollow in most cases—condensed into layers, and to revolve on their axes throughout a great part at least of where their diameters are greatest. This mode of formation seems to throw light on some of the phenomena that have been observed in comets. We have just said that our peak would be divided into several parts, so if we suppose the leading part of it to have been made into a comet, we can see why its tail should have the appearance of a hollow cylinder ; and there might be no reason why the second division, or even the third, should not become a comet also. Then for further divisions, where the diameter came to be too small to make a comet, its matter might have formed itself into a meteor-swarm, and account for the fact of some comets and meteor-swarms revolving round the sun in the same orbits; perhaps even for some of the observed meteor-swarms being denser at one part than another, owing to two or more of the sections of the peak following each other at some distance. We have to notice, after what we have just said, that it is quite possible that if the different sections of our peak did come to revolve round the sun, their perihelion distances might be so different that it would be impossible to trace any connection between them and the peak from which they were derived. But if we were to attempt to set forth all the explanations of the phenomena of comets and meteor-swarms that have occurred to us, there would be no end to our labour.

Passing now from one to the whole host of peaks, we have seen that at one time they projected from all sides of the nebula; it is clear, therefore, that the bodies formed from them must have fallen in towards the sun from all directions, which is exactly what they have been found to do. Then, if we think of the multitude of them there would be, we have also to think that there would most certainly be collisions among them, which would smash them to atoms, and thus help to make the plenum, or host of independent meteorites that are supposed to exist, or would be swallowed up by the sun in mouthfuls. Others might coalesce, which they could only do through coming in from slightly different directions and with nearly similar velocities; and they would thus account to us for comets with a plurality of tails. Again. looking back to what we have just said of the form that might be assumed by the leading end of the peak *a* Geminorum, which was suggested by Donati's comet, we could imagine another, the same in almost all respects, coalescing with it, and between the two showing us how Coggia's comet was formed. Furthermore, with respect to one of the gigantic comets with endless tails: If we suppose it to rotate on its axis, and to be not so smooth on its outside as a cone formed in a turning lathe, we could account for the light from the sun reflected from it having an appearance of flickering ; and, were the outside very rough, for the reflected light flashing from millions of miles of its length in a few seconds.

All this about nebular peaks, comets, etc. formed from them, will, far more than likely, be looked upon as imagination or speculation run mad ; but if it is looked into properly, it will be found that no part of it is based on assumption ; farther than that, the universe has been formed out of cosmic matter of some kind. There is no step in the whole process, from cosmic matter to the sun—even myriads of suns—that does not conform to what are generally called the laws of nature ; whereas it is not difficult to show that some other speculations on the same subject have never been carried beyond the stage of conception.

When thinking of how comets might be formed, we could not help thinking of their orbits and periods of revolution. It was easy to see that their orbits depended on where, and how far, they came from ; that the where might be from any and every direction, and that the how far would be the principal element in their greater or lesser ellipticity, which could only be determined by measurement ; but their periods of revolution, as far as we can see, could only be determined by observation, which would involve the study of several revolutions. On these points the data we have been able to collect are not very satisfying, neither are they given to us as very reliable, except as to those whose orbits have been often observed and measured ; and even among these the orbits are said to vary, and some of the comets to disappear altogether. Again, some of them are said to have a disposition to become associated with particular planets ; and yet again, some people have gone the length of supposing that they have been ejected from some of the planets. To us it seems much more rational to suppose that the known periodical comets have been made out of part of the multitude of peaks which must have surrounded the nebula at one time, if the sun was formed out of nebulous matter, subject to the attraction of similar matter surrounding it on all sides. It seems to be only a way of getting out of a difficulty to suppose that matter ejected from, say the earth, with a velocity of 7 miles per second would be freed from its attraction, that it would be involved somehow in the sun's attraction, and that it would revolve thenceforth round the sun like any other wanderer ; because we cannot see what would stop its progress upwards, so to speak, from the earth after getting beyond its control, or communicate to it at the right height and time, the exact velocity required to make it revolve for ever afterwards round the sun ; nor, supposing the sun would have nothing to do with it, where it would go. When it left the earth, it might have a direct motion of near one-third of a mile per second derived from its rotation, and also one of 18 miles per second due to the revolution of the earth round the sun. It might also be ejected in a direction exactly away from, or directly towards the sun ; so we should have two very different cases to reconcile in order to set up the theory of ejection of comets from planets, and of their being involved somehow in the sun's attraction. It presents us with a very strong case for calling for either the immediate intervention of some power other than what we conceive attraction to be, and of which we know nothing physically, or we have to trust in *man*ipulation of which we have no very exalted idea. We prefer to look upon the formation of all comets as derived from the peaks we have been treating of, or, if that is inadmissible, from shreds and patches of the original nebula ; where no immediate intervention, or instant application, of supernatural power is required, but only the even and tranquil operation of original design.

For comets larger, and which travel to greater distances, than those alluded to above, it is very difficult to get data on which we can form satisfactory calculations of the lengths of their orbits and mean velocities of revolution, for there is almost always awanting some one or more of their elements, or totally different statements given of their value ; but we think we have found a few from which we can collect data sufficiently accurate to enable us to show that there is no necessity for going beyond the domains of the sun, as described by us in a former chapter, to account for any one of the comets which have been taken notice of in astronomical history; and still less necessity to suppose that any of them have wandered, or been shot forth, from some neighbouring star into the solar system.

From the data we have been able to collect it would appear that when a comet comes to have a period of over 70 years, it is either too far removed from the sun at its aphelion passage, or its mass is too great for it to be perturbed by the attraction of any of the planets. For instance, we have Halley's comet, which has been observed for not far from 2000 years, whose period has averaged very close upon 77 years during the whole of that time, showing that it has not been perturbed to any appreciable extent when near its perihelion passage. No doubt 2000 years is a very small period of time to judge from, and its aphelion distance being only 3,258,000,000 miles, it might be influenced to some extent by some planet, so we can hardly count upon its being permanently exempt from perturbation. Indeed, Halley himself supposed that its velocity of revolution had been considerably increased when it was in the neighbourhood of Jupiter in the interval between 1607 and 1682; but if it was so, there must be some counter-perturbation which restores the balance so as to make the average period of 77 years. Looking over the register of its appearances, we find that in its re-appearances of the years 66 and 1758, the period was about 75 years, and that in those of 451 and 1066 it was 79 years; so that if there are perturbations, we must claim that there are also compensations. Seeing, then, that we can find no evidence to the contrary, we may suppose that when the periods of comets, and, perhaps more especially, when their aphelion distances reach to beyond—and the farther the more so—the orbit of the most distant planet, they may be looked upon as not being liable to be seriously perturbed by any of the members of the solar system, until something to the contrary had been proved. Following this idea, then, it occurs to us that something may be learnt from their mean velocities in their orbits, as will be seen from the following very small list of those we have been able to submit to calculation, which form the accompanying

TABLE XI.—SHOWING THE MEAN VELOCITIES IN ORBIT OF SEVERAL COMETS.

Designation of Comet.	Aphelion Distance in Miles.	Period of Revolution in Years.	Mean Velocity in Orbit per Second.
		years.	miles.
Halley's comet . .	3,258,000,000	77	4·18
Comet of 1532 and 1661 .	4,464,000,000	129	3·45
Donati's comet . .	13,873,280,000	2,000	0·69
Comet of 1811 . .	40,000,000,000	3,065	1·3
Comet of 1680 . .	78,468,852,000	15,864	0·49

These orbital mean velocities per second have been calculated from aphelion distances as diameters and from circular orbits, which probably give results rather lower than would be derived from elliptical orbits—were they known—but on the other hand, the perihelion distances have not been taken into account in fixing the diameters—because they were unknown—so the error will be so far compensated, if not altogether.

We know that the mean velocities in orbit of the planets decrease as their distances from the sun increase, and our table, as far as it goes, leads us to believe that the same holds good with comets whose aphelion distances are comparable to those of the planets, in being measured by hundreds of years or less of revolution; but with those whose periods are measured by thousands of years, the same rule seems to fail. One thing, however, that we seem entitled to believe is that, generally speaking, the greater the period of revolution of a comet is, the less will be its mean velocity per second in its orbit. It will be observed that the average mean velocity of the three remote comets in the table is only 0·83 mile per second, and it is by no means unreasonable to suppose that the average mean velocity per second of any number of comets whose aphelion distances are greater than the highest of those in the table, is not likely to be so great as the average of the three ; on this understanding, then, let us take, or suppose, one whose mean velocity in orbit per second is only one mile, and look into what may be learnt from it.

Going back to the peak of *a* Geminorum which we supposed, at page 321, to be condensed to 129,000 million miles in diameter of base, its height $1\frac{1}{4}$ billion miles, and distance from the sun 11 billion miles, we may take a comet formed from it as an example. If, then, we suppose the leading part of it to have been formed into a comet with that aphelion distance—11 billion miles—and other dimensions suitable to its new condition ; taking its mean velocity in orbit at 1 mile per second, we find that its period of revolution might be 1,200,000 years, or three times greater than that of the comet of 1882, namely 400,000 years, mentioned by Mr. Chambers as being not very reliable, probably because its angles in orbit could not be measured with sufficient accuracy. Then, when we think that the sphere of the sun's attraction in that direction—of *a* Geminorum—extends to 67 billions of miles, and that there are stars more than 6 times farther off, e.g. Canopus, see Table VII., we see that a supposed comet might have an aphelion distance equal to that ; and were we further to consider that were its major axis 67 billion miles long. including aphelion and perihelion distances, and that it went straight from the one end of it to the other and back again, its period of revolution, if it could be so called, would be 8,500,000 years ; that is 20 times greater than Mr. Chambers's doubtful 400,000 years for the comet of 1882. There seems, therefore, to be no necessity for the solar system sending its cometary produce to a foreign market ; and our mechanical imagination is not sufficiently vivid to allow us to conceive what kind of potential energy even Jupiter can have to give an impetus to a comet, great enough to send it flying to so great a distance. What velocity would it have when it left the sun ? And what would remain in it to carry it over the debatable land between the sun and a distant neighbour ? Or are we to believe that all the solar system's produce of that kind is only sent over the channel, as it were, to our nearest neighbour, *a* Centauri ? Conceptions of that kind are too elevated for us, and we must leave them alone. Mr. Chambers expresses doubts as to the determination of whether the orbit of a comet is elliptical or parabolic when its period of revolution is measured by hundreds of thousands of years, and we think we are safe in following him until actual proofs are presented. If the comet of 1882 never comes back, we may then believe it has gone elsewhere.

Having used up all the nebulous matter in the sun's domains, as described at the beginning of Chapter XV., or at least shown how it may have been, or may yet be, used up, we have now only to make a few remarks to prove that our description of the said domains is not by any means fanciful. It matters very little whether the solar system was begun to be brought into existence at the same time as the surrounding systems or before or after them. What is certain is that the sun's sphere of attraction among its neighbours is bounded, at the present time, just in the way we have taken to describe its domains. How they were filled with cosmic matter may be disputed, but filled they must have been somehow, if the solar system was formed out of a nebula; and the way adopted by us was the only one that occurred to us when we began to reconstruct the original nebula. Since then we have had time to reflect on our work, and to see how it points out the simplest way that can be conceived, which may be expressed in the few following words. We may suppose that the ether was the primitive matter, as we have done at page 258, and that the whole material universe has been formed from it and through it. This idea will assist physicists in forming their theory of a plenum of meteorites or meteoric matter, if such they choose to call it. It will also enable us to complete the circle of our notions with respect to matter. We believe that we can neither destroy nor produce the smallest portion of it, although we can change its form. Thus, looking upon the ether as primitive matter, we can understand how the solar system could be elaborated from it; and how, after having accomplished the purposes for which it was brought into existence, it may again be resolved into the primitive element out of which it was made, ready to take its part in the evolution of some other system with, perhaps, a new earth "without form and void."

We have now to direct our thoughts, as far as we can, to the mass, which furnishes the really effective power of the sun as the ruler of the system; and, first of all, we have to think of what are the real active elements which form that mass. Hitherto we have looked upon them as all included within a diameter of 867,000 miles, but now we have to take notice of the clouds of meteoric matter which have been supposed by some astronomers and physicists to be revolving round the sun and continually raining into it; and of the enormous atmosphere which surrounds it. With regard to the former of these two elements, we shall compound our ignorance by looking upon it as a merchant does on his account of Bills Receivable, as not being available in the case of a sudden demand for cash, and therefore as not forming a part of the mass, any more than as the attraction of the earth aids the sun in its management of the planet Neptune; the same as the bills receivable strengthen the credit of the merchant. But with regard to the second element of the two, we must recognise that it forms part of the mass and power over the whole of the system, and from all that is known about it we are not authorised to look upon it as a negligible quantity. It so happens that the only thing we have to which we can compare it is the atmosphere of the earth, and we immediately find that there is absolutely nothing to be learnt from such a comparison. We know that one-half of the weight or mass of the earth's atmosphere is contained in a belt of $3\frac{1}{2}$ miles high above its surface, so that double the volume of that belt estimated at atmospheric pressure gives us the true measure of its mass. This mass, when reduced to the density of water, and compared to that of the earth as we have dealt with it all along, turns out to be about $\frac{1}{824,000}$th part of it; and were we now to add that to the earth's mass we have been using, its mean density would be 5·66065 instead 5·66 times that of water.

Now, let us suppose the sun to have an atmosphere of the same kind as the earth's: Seeing that the force of gravity at its surface is about 28 times greater than it is at the surface of the earth, a belt around it which would contain one-half of its mass would be $28 \times 3\frac{1}{2} = 98$ miles, or say 100 miles thick. Dealing then with this dimension in the same manner as we have done in the case of the earth, we find that its supposed atmosphere would be $\frac{1}{836{,}000}$th part of its mass, which, if added to the mass we have used for it, would make its mean density 1·413016 instead of 1·413 times that of water. Then again, if we suppose the earth's atmosphere to extend to 100 or 200 miles above its surface, the supposed atmosphere of the sun would extend to 2800 or 5600 miles above its surface, according to which of the above heights on the earth is adopted; whereas the highest of our authorities say that the corona, or apparent atmosphere, extends to at least 350,000 miles from its surface.

It would appear then that there is no analogy whatever between the atmospheres of the sun and the earth; but there must be some analogy, because the law of attraction cannot be suppressed at the surface of the sun; neither can any vaporous matter near it cease to be attracted in the same proportion as it is at the surface. Our atmosphere causes a pressure of 29½ inches of mercury at the earth's surface, and the attraction of the sun at its surface must cause a pressure equal to nearly 28 times that without fail, i.e. 420 lb. per square inch instead of the 15 lb. of the earth. We know that some spectroscopists believe that the pressure at the surface of the sun is sometimes as low as it is at the surface of the earth, even lower; but we require an explanation of why it is so. At the surface of the sun one second of arc corresponds to a height of 450 miles above its surface, and Mr. Proctor states in his "Sun," page 295, that if even "two or three hundred miles separated the lower limit of chromatosphere from the photosphere, no telescopes we possess could suffice (when supplied with suitable spectroscopic appliances) to reveal any trace of this space. A width of two hundred miles at the sun's distance subtends an arc of less than half a second; and telescopists, who know the difficulty of separating a double star whose components lie so close as this, will readily understand that a corresponding arc upon the sun would be altogether unrecognisable." We can understand this, and perhaps find an explanation for ourselves.

According to our supposition that the sun may have an atmosphere similar to the earth's, at one hundred miles in height it would be reduced in pressure to 14 atmospheres, and, extending the analogy, at 2800 miles high the pressure would still be equal to one-eighth of 28 atmospheres, or equal to something less than 2 lb. per square inch at the surface of the earth ; so that if spectroscopists have measured the sun's atmosphere at the disk, and found it to be lower than the earth's at its surface, their results must have been caused by some fortuitous circumstance which they did not notice at the time ; because the force of attraction at the surface of the sun can never be overcome except by some counteracting force, which, if in the form of a vapour, or what we call a gas, issuing from its interior, would increase rather than diminish the pressure. We know that in the heart of a cyclone on the the earth there is sometimes a vacuum sufficient to explode (pull out the walls of) houses near which it passes ; and, at the same time, we know, more or less, what heat the sun sheds upon the outer atmosphere of the earth, and also the rate of rotation of the earth in the regions where the fiercest of these cyclones occur, the only two causes which can produce them. Now, if we compare these causes in the two bodies, that is, the earth's rotation of about 16 miles per minute and the sun's of, say, 60 to 75 miles per minute, and the temperatures of the sun and the earth at their respective surfaces, we can imagine that in the heart of a cyclone on the sun there may be a vacuum much nearer absolute zero than there can be in any one on the surface of the earth. If then the spectroscopists, without knowing it, have caught the spectra of the hearts of cyclones, we can conceive them to be right, otherwise no.

Again, we know that when big guns are fired off partial vacuums are formed near them, sufficient to cause disaster to windows, doors, and even walls of houses too near them, but whatever we may have said of force sufficient to produce explosions in the sun, we have never believed that matter is ejected from the sun by explosions. We have supposed the sierra, or chromosphere, to have oozed out through its pores, sometimes to less, sometimes to greater heights, like steam from an open boiler, and the prominences to be eruptive, neither of which modes could produce anything approaching to vacua in their neighbourhoods. There can be no resemblance between the ejection of matter or gas from the sun and from a cannon, but there is between the ejection of vapours and the escape of steam from the safety-valve of a closed steam boiler; both of them continue to pour out their vapours till the pressure within falls down till it is equal to the resistance to their escape; there is no explosion, therefore no vacuum, appreciable at least, in the neighbourhood. There may be surrounding matter drawn up by the velocity of the outward current, but that is all.

Notwithstanding all this, we see no reason why the sun should not have an atmosphere of exactly the same kind as the earth's, composed of exactly the same kinds of gases, including vapour of water in some part of it, though, perhaps, far removed from the photosphere. Every other element found on the earth can be found in the sun, and so it is not unreasonable to suppose that the same kind of atmosphere may exist upon it; we have only to acknowledge that its conditions must be somewhat varied, all the difference being that the atmosphere of the sun must be heated up to the temperature of the photosphere where it comes in contact with it, while that of the earth is only of the temperature of the earth at its surface. In the case of the earth, if this were at a white heat, one-half of the weight of its atmosphere would not be comprehended in a belt around it of 3½ miles thick. That balance of mass might take place at a height of even hundreds of miles—we have no means of calculating how high—and still its pressure at the surface would be the same as now, as long as the earth's attraction remained the same; so must it be with the sun. Instead of limiting its height to 5600 miles at the utmost as we have done above, it would be no stretch of imagination to suppose that it might extend to ten, twenty, or more times that height. In addition to this we have to take into consideration that the sun's atmosphere must be swept up to something far beyond 5800 miles high by the whirlwinds created by the velocity of rotation at its surface, the same as we saw the earth's might be when we were explaining how an aurora could be made to glow at heights far beyond what we were accustomed to believe its atmosphere could reach. Adding, then, together these two motive forces for elevating the atmosphere of the sun, it would be a bold assertion to say that it cannot have one exactly similar to the earth's, reaching up to the height of 350,000 miles mentioned a few pages back. And now, having got this length, we may venture to assert that the corona of the sun is made up of this atmosphere, and of the vapours of the elements thrown out from its interior, somewhat in the manner we have described in last chapter; to which we have only to add that the bubbling up of vapours all around the sun, which produces the sierra or chromosphere, would not be interfered with in any way by the tremendous commotions which we have shown must be produced between the surfaces of the sun-spot zones and the centre; and that the projection of the high prominences would assist in elevating the aeriform atmosphere.

If then the sun has a compound atmosphere of this kind, it must be considerably more dense, proportionately, than that of the earth, and will consequently form a greater addition to its mass than we have found would be made by its airlike atmosphere. But, whatever density has to be added to it on that account has to be subtracted from the interior having been ejected from thence; because, in whatever manner its mass has been calculated in respect of the other members of the system, the total amount must turn out to be always the same. We have always estimated its mass from a diameter of 867,000 miles, which gave us a volume of $341,237,638^{9}$ cubic miles, so that if we now include in the diameter the 350,000 miles height of the atmosphere, we get a volume of $2,053,500^{12}$ cubic miles, which is as near as possible six times the volume in which we had to distribute the volume of the sun. How to do this, we know not. We cannot fix the region of greatest density in the same manner we have done at page 221, but we know that it must be considerably nearer to the surface of the photosphere than we have there placed it ; and of one thing we are sure, and that is, that the densities we have named for that region and the outer and inner surfaces of the shell, at page 223, must be less than those there expressed ; how much we cannot calculate, but we have certainly found that the limits must be lower, and that most probably there is no matter in the sun exceeding the half of the density of water.

Whatever the composition of the sun's atmosphere, or corona if that name be preferred, may be, spectroscopists have found in it a *spectral* line derived from some substance totally unknown to science. Now, looking back on our work from almost the very beginning, it seems to have been gradually borne in upon us that this unknown substance is the ether. That it is a material substance we were hardly ever in doubt, and our studies of it have substantiated and confirmed our belief. In our analysis of the Nebular Hypothesis in Chapter VI., after combating the notion that the light of nebulæ is occasioned by incandescent gas, we showed, by the example of an air furnace, that an incandescent gas is composed of two elements, one consisting of solid matter which takes up and gives out heat and has all the properties of a heated solid or liquid substance, and the other of gaseous matter which, being the element that fills up the empty spaces between the solid atoms of a gas or vapour, only performs the office of carrying the solid part into the furnace. This forced upon us the idea of the gaseous part being a carrying agent, and very naturally to think of its being really the ether, that being the only acknowledged agent for the carriage of light, heat, and electricity, two of which are easily seen and felt, and the third cannot be awanting, in an air furnace. Again, when treating in Chapter VII. of what effect the ether might have on the density of the original nebula, we concluded that its density must be much lower than what we then knew it had been estimated to be, and also that its temperature in space must be lower than -225°; which two circumstances combined showed us that if it is a gaseous substance it must be very different to any gas that had been liquefied up to that time. This we repeated in great part in Chapter XII., calling attention to the peculiarity of its being able to carry a higher temperature than its own—to all appearance—into a "hot box." Then we have dedicated two Chapters, XIII. and XIV., almost exclusively to the study of the ether, and have been led from one stage to another to look upon it as the only substance that agrees with the definition of a gas as given by science ; true gas there is ; as the primitive and sole element in the formation of all matter and in the evolution of the universe ; and what is something more than an unfounded guess, as the mysterious and incomprehensible agent attraction, unfortunately almost universally spoken of as gravitation. And now to conclude: From what we have been able to learn, very slight differences have been found in various spectra of the position of the line representing the unknown substance, but this can cause very little doubt of its always being the same, as spectra often contain several lines of hydrogen, owing most probably to combinations with other substances ; and if the ether is the primitive chemical element, there may be slight differences in the position of its line, as shown in all the phases in which we seem to have found it, but they must be slight as compared with the hydrogen lines, because even these must be in some measure, perhaps even great, influenced by the unfailing and inevitable mixture of the ether in their composition.

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